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MDCCCLXXXIX.

THE PHYSICIAN AS NATURALIST

ADDRESSES AND MEMOIRS BEARING ON THE HISTORY AND PROGRESS OF MEDICINE CHIEFLY DURING THE LAST HUNDRED YEARS

BY

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TO THE MEMBERS OF
THE BRITISH MEDICAL ASSOCIATION,
AT HOME AND ABROAD,
The Author
RESPECTFULLY DEDICATES THESE PAGES,
IN RECOGNITION OF THE GREAT HONOUR CONFERRED
ON HIM IN AUGUST, 1888, WHEN HE WAS CALLED TO OCCUPY
THE CHAIR OF THE ASSOCIATION IN GLASGOW, AND
TO DELIVER THE PRESIDENTIAL ADDRESS.

PREFACE.

THE memoirs contained in the present volume form a portion—perhaps the portion most likely to be generally interesting—of a much larger number of scattered contributions to the literature, scientific and practical, of medicine during more than thirty years past. The author has been engaged during all of these years, and more, in teaching the medical art, and has thus had occasion to note very particularly, in the interest of his pupils, the changes—even the revolutions—in opinion and practice that have taken place within the compass of two generations. The effect of this upon his own mind has been that he has never found it necessary, or expedient, to formulate for teaching purposes a scheme of medical doctrines, a system or general theory of medicine, such as may be found abundantly in the records of the past. He believes, however, that in these papers, though written at widely different times and on various occasions, there will be found a fairly consistent purpose and undercurrent of thought, which may perhaps stand in the place of such a dogmatic system; and that he may be justified in referring to this volume as containing principles that have guided the academical and practical instruction of very many successive groups of students, during a period of service longer than has been allotted to many teachers, in two of the largest medical schools in this country.

It ought perhaps to have been stated that the earliest in date of these papers, the essay on Homœopathy, was written for a volume of *Edinburgh Essays by Members of the University*, published in 1856. This essay led to some controversy at the time, but has occupied a very small place in the author's consideration either at the time or since. It has now been revised in accordance with a desire to remove whatever may have been, even in form, open to serious criticism; and thus to present the matured convictions of its author.

GLASGOW, *January*, 1889.

CONTENTS.

PAGE

I. THE PHYSICIAN AS "NATURALIST," - - - - 1

Appendix.—Extract from Introductory Address delivered in the University of Glasgow to the Students of Medicine of the Session 1882-83, - - - - 55

II. HAS THE ART OF MEDICINE ADVANCED DURING THE LAST HUNDRED YEARS, - - - - - 66

III. FACTS AND CONCLUSIONS AS TO THE USE OF ALCOHOLIC STIMULANTS IN TYPHUS FEVER, - - - 122

Leading Article from *The Lancet* in reference to the above, - - - - - - - - - - 149

Note on the Changes described as occurring in the Epidemic Fevers of Edinburgh, 1826-29, - - - 154

IV. COURSE OF TYPHUS FEVER AND PHENOMENA OF THE CRISIS, - - - - - - - - - 158

Note as to various opinions on the Alimentation of Patients in some Febrile and in some Chronic Diseases; and especially upon the Uses and Abuses of a Milk Diet, 184

Note on the Thermometer as a Guide to Prognosis and Treatment in Typhus, - - - - - 191

V. TWO MONTHS OF FEVER DUTY IN THE GLASGOW ROYAL INFIRMARY, - - - - - - - 193

VI. ON THE LIMITS OF ALCOHOLIC STIMULATION IN ACUTE DISEASE, - - - - - - - 208

PAGE
VII. MIND AND BODY, - - - - - - - - 245

VIII. SANITARY SCIENCE AND PREVENTIVE MEDICINE, - 266

IX. THE PROGRESS OF PATHOLOGICAL SCIENCE; JOHN BAPTIST MORGAGNI AND HIS SUCCESSORS, - - 294

X. HOMŒOPATHY, - - - - - - - 315

XI. THE PRESENT ASPECT OF MEDICAL SCIENCE—OLD ORTHODOXY AND MODERN OPINION, - - - 370

XII. DR. ALISON, - - - - - - - - - 388

INDEX, - - - - - - - - - - 431

I.—THE PHYSICIAN AS "NATURALIST."

In seeking about for a topic[1] on which I might occupy your attention this evening, I have been led to make some special reflections on the curious survival among us of an ancient way of thinking that is presented to the mind by the designation, in English, of the *physician,* or, as Chaucer has it in his well-known Prologue to the *Canterbury Tales,* the *Doctour of Physike.* I do not know if it has occurred to many of you to observe that in no other language than our own has this survival occurred. The surgeon—*chirurgus*—has, indeed, kept from a very remote antiquity the title which was given to him in the days of his subjection as the *handworker* or *operator* under the direction of the physician. But the most remarkable thing about this last, and conventionally the higher, title is that while it seems to recall a time when the medical art was distinctively associated in the minds of men with the study of φύσις, and when the healer of the sick was regarded as in a very special if not exclusive sense a *student of Nature*, it is very hard to discover from the traditions of language, either our own or any other, when this idea first took shape—how and when the notion began to be entertained that the most fitting title for the most highly honoured representative of the medical art was to call him, distinctively, a *naturalist*, or, if you will, a natural philosopher or *physicist*. It has occurred to me that it may not be an altogether unprofitable task for one who holds a Chair in this University popularly designated as one of *Physic*, to inquire how the idea represented in this word *physic* came first into existence, and how it got floated into such a degree of popularity as not only to have practically displaced to a considerable extent in our own language the much older one of *medicine*—that is, healing—as applied to the art itself, but to have got itself into currency as applied to the very tools of the art, the drugs with which the physician, so called, was supposed mainly to work his cures. It is surely a very marked instance of what the late Archbishop Trench called the *degeneration* of words, that in the course of ages all that nasty stuff which the Doctor Slops of the past made it a duty to send remorselessly down the throats of the lieges, whether in the form of pill, powder or potion, should have got itself called by the name of *physic*—that is, Nature's remedies; while it is the very irony of historical fate that this word, in this sense of it, should now be current in these islands only. "Throw physic to the dogs, I'll none of it," was not a phrase that would bear literal translation even in Shakespeare's time, I believe, into any other modern European language; but it perfectly represents the tradition of our own language in the time of Shakespeare, a century or more after Henry VIII. granted their charter of incorporation to the Royal College of Physicians in London—a tradition which continues active up to the present hour.

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[1] The Address, which is here reprinted, was delivered on 7th August, 1888, as the Annual Presidential Address to the British Medical Association, meeting in Glasgow. Several pages, referring to local and personal matters, are here omitted, but will be found in the *British Medical Journal* for 11th August, 1888.

If I propose to you to look with me a little into this historical question, it is not as a mere academical exercise in philology, though even from that point of view curious and interesting enough; but because it contains within this a further inquiry which, even in this nineteenth century, may be turned to profitable account in an assembly like this. I hope to be able to show you that these words *physician* and *physic* have relations with some of the very best and highest traditions of antiquity, and that it may be possible for us even now to make an application of them which will repay and at the same time justify their retention in the English language, although, it may be, tending also to discover certain deficiencies which still unhappily exist in our systems both of medical and general education.

My argument, in other words, will be this:—For a series of indeterminable ages, from the time, very probably, of Hippocrates downwards to what we call the Dark or Middle Ages, the tradition has continuously existed that the *healer* or physician of the highest class ought also to be, in a very real sense of the word, a *naturalist* or perhaps a man of *science* (physical science being, of course, understood); that it is his prerogative to be trained and exercised after the best manner and according to the most thorough discipline of the science of his age; and that he ought to be (or, at least, that he has been in very remote times) regarded as being admirable and trustworthy as a healer or physician, chiefly in proportion to the confidence reposed in him as a *naturalist*, that is, a humble, reverent, and exact follower and student of Nature.

THE HIPPOCRATIC TRADITION.—ANTICIPATION OF THE "NOVUM ORGANUM."

You are all familiar, no doubt, with the magnificent opening of the *Novum Organum*,[1] which ascribes to Man as the "minister (or servant) and interpreter of Nature," only so much either of power or of knowledge as he has gained by observing the order of Nature, outside of which he neither knows nor can do anything. Now, it is a curious fact, which has not escaped the editors of Bacon in recent times,[1] but which may require, nevertheless, to be brought to your notice, that the very word or phrase here used to designate the limitations imposed upon the *power* of Man in reference to Nature is the one which, in a very remote age, had suggested itself to Hippocrates as specially indicating the function of the healer. He is, he must be (according to Hippocrates), "the servant of Nature"—*ὑπηρέτης φύσεως*. Nor is this a mere accidental expression, which might be passed over as a coincidence not extending below the surface. On the contrary, the expression is taken up and specially commended by Galen (surely the best of all authorities on such a point) as being of the very essence—the key-note, as it were, of the Hippocratic teaching, with which all the later authorities (Aristotle and the Peripatetics), as he tells us, were essentially in accord. So that, as we are now being taught that Bacon was the real author of Hamlet, it might be a perfectly fair, if not actually verifiable, literary retribution to maintain that Hippocrates initiated the *Novum Organum;* or, at least, that the root-principle of all that is best in that great fragment was not only suggested but actually expounded and worked upon as the true method of medical science and practice more than two thousand years before. That Bacon should have derived this remarkable expression from Hippocrates (though he nowhere says so) is far from improbable, inasmuch as, scattered through his numerous writings, there are many evidences of his acquaintance, more or less exact, with the father of Greek medicine; of whom, in more than one place, he particularly notes the "ancient and serious diligence" in recording special cases, as opposed to the "much iteration and small addition" of medical science in general, which, he says, has been "more professed than laboured, and yet more laboured than advanced; the labour having been, in my judgment, rather in circle than in progression."[1] But, in truth, Bacon's animadversions on the medical art are (as Harvey said of them) very much after the manner of a lord chancellor; and it is perhaps less of a marvel that he should have failed to indicate Hippocrates as the author of one of his own most pregnant sayings, than that he should have entirely overlooked and neglected the discovery of the circulation of the blood, which was being made and published under his own eyes; the direction of his thoughts as to medical progress, meanwhile, being in no single instance, I think, such as would approve itself to a modern nineteenth-century physician, or would appear to him to contain even the germ of any substantial improvement.[1]

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[1] "Homo, Naturæ minister et interpres, tantum facit et intelligit quantum de Naturæ ordine re vel mente observaverit, nec amplius scit aut potest."—Aph. i.

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[1] See note at p. 157 of Ellis and Spedding's edition of Lord Bacon's works, vol. i., 1870.

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[1] *Advancement of Learning*, Book 2; compare *De Augmentis Scientiarum*, Lib. iv.; but a far more depreciatory estimate of Hippocrates is to be found in *Temporis Partus Maximus*, chap. ii.; Ellis and Spedding's edition, vol. i. 591, vol. iii. 373, 534.

But to return to Hippocrates and his remarkable declaration, that the *ἰατρός*, or healer, is the servant of Nature (*φύσις*). This expression, as I have already said, is no merely casual one in the writings of Hippocrates; for, in one of the two places[2] in which Galen remarks upon it (probably six hundred years afterwards, and in the full knowledge, therefore, of all that could be said for or against the expression by rival sectaries) he does not hesitate to declare that Hippocrates was "*the first* to observe the works of Nature;" and that he "is always admiring and insisting upon the sufficiency of Nature, whereby what is necessary for the life of all animals is done *ἀδίδακτος*, that is, spontaneously and without apparently conscious effort." He thus places Hippocrates distinctly in advance of, if not above, Aristotle and the Peripatetics, in respect of originality in the study of *φύσις*; and he further maintains that Erasistratus, the Alexandrian anatomist, had adopted an inconsistent attitude towards Nature, and that his followers had exposed themselves to ridicule by their unintelligent criticism on what was simply a development, by the Peripatetics, of the *physiology*, that is, Nature-teaching of Hippocrates. It is not necessary to go into this old controversy now further than to show that, by the very fact of its having become a controversy at all, the position of the ἰατρός, or healer, as the "servant of Nature," must have been very well known not only to Galen but probably also to Aristotle; and through these to the Arabian physicians and to the whole of the Middle Ages, of which they were the teachers and lawgivers. And while a good deal of Galen's commentary is of course antiquated and little instructive for us here, there is a curiously modern look about one part of it, which shows that exactly the same great evils which have grown up about the art of medicine in later times, and on which I had something to say in my Dublin address of last year, had already become rife in the second century of the Christian era, long before the remoter East had sent us that long array of outlandish drugs, or *physic* (popularly so called); and with these the many ponderous tomes of medical philosophy, which were by and by re-translated from Arabic into Latin, and thus exercised a dominion alike over the scholars and physicians of the Middle Ages. Hippocrates, in Galen's opinion, was eminently right in calling the physician the servant of Nature; but this expression, he adds, " will apply only to the true *healer* and not at all to the mere *druggist*, who might more properly be called the enemy and adversary both of Nature and of the sick. For the healer acts only in view of the more continuous processes of Nature, in her innumerable ways of preserving the bodies (of animals), which (adds Galen), if I were to recount here, I should have to transcribe the whole of my treatises on the *Uses of Parts*. Therefore (he adds) it is not to be wondered at that Nature should be said (that is, in the place of Hippocrates on which he is commenting) to be all-sufficient; for he himself has written elsewhere, 'our natures are the healers of our diseases.' "[1]

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[1] See the criticisms above referred to on the science of medicine, and in connection with some of these, the very singular exhibition of Bacon's own mind upon the subject in detail, in the *Historia Vitæ et Mortis*, vol. ii.; and the Medical Remains at the close of vol. iii. of Ellis and Spedding's edition.

[2] Galen, *Op.*, edit. Kuhn, xv. 309, xvi. 35.

Nor did Hippocrates, the Father of Medicine, escape the reproach which it has been so easy and so profitable in many ways to fling at those who, in a later day, have proceeded in accordance with his precept, if not his example. We hear chiefly from Pliny and Cælius

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[1] Galen, *Comm. on Hippocrates* περὶ τροφῆς, xiv. The isolated position of this last expression (so often ascribed to Hippocrates, though occurring in a work very certainly post-Hippocratic in date) might expose it to the suspicion of being an after-thought, and perhaps a graft from the Peripatetic or Aristotelian philosophy of " Nature " ; but Galen's reference here shows that he, at anyrate (by far the best authority we have on what was, or what was not, Hippocratic), recognized the often-quoted "vis medicatrix naturæ " as being distinctively on the lines of the older Hippocratic tradition. I may take this opportunity of acknowledging my great obligations to that most eminent and most admirable of medical scholars, Dr. Greenhill, formerly of London, now of Hastings, for some of these references.

Aurelianus of a certain Asclepiades, of Bithynia, a contemporary of Cicero, whose true character it seems rather difficult to decipher, but who at least may be said to have been a fashionable physician in Rome, with a brand-new system of his own. Asclepiades, whose *rôle* in the treatment of disease seems to have been one of constant interference, or, as we should say, of meddlesome physic, only (it is believed) using, as a rule, and in a temporizing kind of way, the mildest and most agreeable of medicines,[1] had, of course, no appreciation at all of any one who, in his character of a healer, professed to be a "servant of Nature." He said, in fact, that, in speaking of Nature as a kind of intelligent principle, Hippocrates was (not to put too fine a point upon it) talking nonsense. Nature is too often bent, not upon healing the man, but (as a witty member of this Association once said in my hearing) on putting him into his coffin! Hippocrates, as the servant of Nature, is simply a *waiter upon death* (θανάτου μελέτην). The true business of a physician is to "make himself master of the occasion;" that is, to shove old dame Nature out of the way, perform the cure *tuto, cito, et jucunde,* and claim all the credit; which, no doubt, he did in Rome, as the quacks in all ages have done everywhere, with great comfort and advantage to himself, and (let us hope) with the minimum of injury to his patients.

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[1] He is reported to have been the inventor of the phrase: *tuto, cito, et jucunde,* as applied to medical treatment in general. But he also employed, according to Pliny, magical remedies to a great extent. (See Le Clerc, *Histoire de la Médecine,* 2nd partie l. 3, cc. 4-7.)

The truth and the falsehood that underlie this old-world argument I will not attempt to discuss this evening; having done so already on more than one occasion. I am alluding to it now mainly to show that the position, in reference to φύσις, of him whom we still call the physician was a well-recognized one long before the origin of the term *physicus,* as applied to him in the Latin of the Middle Ages and the French of the thirteenth century; from which, in all probability, we have derived our English word. What I have now to do is to inquire how far we are maintaining, in this nineteenth century of ours, the position assigned to Hippocrates by Galen (and, I have no doubt, rightly assigned) of being prominent among the seekers into φύσις, or, at all events, capable workers in this field, in accordance with the methods and advances of modern physical science.

THE PHYSICIAN IN THE MIDDLE AGES.

It may be not unimportant for this purpose to remark that, so far as we can judge of him from literature, the physician of the Middle Ages, though retaining the name, was in a very small degree, if at all, cultivated according to the type. Chaucer's Doctour of Physike, though very far indeed from being a pedant, was assuredly much more of a *learned* than of a *scientific* physician. He was "grounded in astronomy (astrology);" he spent much of his own time and his patients' in "magike naturel," which, though probably a very curious type of "physic," must not be supposed to be at all suggestive of the "black art" or of any kind of tampering with the powers of evil.[1] In other respects he was a "very parfite practisour," probably full of good common sense, and genial human nature, well up in the old Hippocratic doctrine of the *humours,* and in close relations with the apothecaries, "to send him dragges and his lettuaries;" but there is no hint of anything at all resembling genuine research (as we now understand it), nor even of the observation of natural phenomena in any really practical fashion. How, indeed, could there be, as we have every reason to believe that the thing at this time hardly existed? The fate of Roger Bacon in the century before Chaucer was an amply sufficient warning to the good-natured and easy going doctors of his time that anything like original research into φύσις was dangerous, nay, liable to be proscribed and punished with imprisonment, perhaps with the fagot, unless it proceeded exactly on the lines of St. Thomas Aquinas, the "angelic doctor." It was very much easier and more comfortable, in every way, to stick to Hippocrates and his "humours," where everything was sure and safe; and to add a little astrology, which at least was permitted, if not encouraged,[1] and could get no one into trouble. And when we come down more than a century and a half to Rabelais, two centuries to Montaigne, three centuries to Molière and Guy Patin, we find the position still much the same, or rather, in all probability, worse, as respects the

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[1] "Natural Magic," in Chaucer's day, was probably only a kind of astrological prognosis, and therewith (as the two succeeding lines show) an application of supposed means of cure resting upon the *arcana Naturæ* as then supposed to be accessible for this purpose. Although the "images" referred to may carry, even to our minds, a suggestion of the practices which were ascribed to the practitioners of witchcraft much later (even in the eighteenth century in the well-known case of Maxwell of Pollok, who was supposed to have been bewitched in some such way) there is no hint of this in Chaucer. But the course of history shows, and the famous Faust legend illustrates, the tendency of the popular imagination to associate "natural magic" with the supernatural; or, as Naudé puts it in his curious apology, to confound "la magie diabolique avec la naturelle." Naudé, *Apologie pour les Grands Hommes soupçonnés de Magie*; especially ch. 2 "De la Magie, et de ses espèces.") Gabriel Naudé, the author of this most interesting and learned little work, was born in Paris A.D. 1600. He studied medicine in 1622-23, and in 1625 published the first edition of this book. He afterwards became librarian to Cardinal Mazarin.

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[1] Judicial astrology, however, which (according to Naudé) was "l'enfant supposé de l'Astronomie," was (he says) very properly condemned by the Church, "non point comme suspect de magie, mais comme une science vaine et chimerique, *quæ stellis ea quæ geruntur in terrâ consecret;* qui veut penetrer dans nos destinés et qui par la témérité qu'elle a de vouloir s'egaler à la Providence, en fouillant dans l'avenir, combat directement la Religion." So that Chaucer's Doctour of Physike was, perhaps, a heretic after all!

physician; although surgery and anatomy may have been making some steps in advance. I apprehend that the doctor of medicine in the middle of the seventeenth century in France, unless he has been caricatured out of all recognition by Molière, must have been altogether the most stupid, pompous, brainless formalist that ever in any age of the world practised the art under a learned title. Scholastic lore, such as it was (and that, too, of a very poor and superficial kind) had eaten out the heart of him, and at the same time puffed him up into the *simulacrum* of a personage, who rode upon his mule with gorgeous trappings, and wore a robe, and surrounded himself with a wall of *etiquette*, which only served to conceal gross ignorance. The best of them, such as Guy Patin, Dean of the Faculty, who has left us a transcript of himself in almost innumerable familiar letters, were men of much conventional learning and good intentions, but wholly without insight, and tied up in chains of routine. The worst of them were, probably, what Molière describes, no doubt with pardonable and amusing exaggeration. Molière, indeed, could hardly fail to find out and make ridiculous for all time the weak points of such a medical *régime* as then existed. Everyone must remember M. Tomès in *L'Amour Médecin*,[1] who, when told as a matter of fact that his patient was dead and buried, replied that *that* was impossible, because Hippocrates had said that this particular kind of case was not fatal except on the fourteenth or the twenty-first day. Crushing rejoinder—"Hippocrate dira ce qu'il lui plaira; mais le cocher est mort!"—the force of learned folly could go no further. The satirical portrait of Thomas Diafoirus, which I have had occasion to quote before now,[1] and the magnificent installation of Argan in the *Malade Imaginaire*, remain for us and for our remotest successors, to show how the art of healing may degenerate under the influence of scholasticism, and how base a

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[1] It is worth while here to give the whole of this amazing dialogue, the point of which, as regards the present discourse, consists in the fact (I believe well-ascertained) that Molière had in

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view in M. Tomès (*i.e.* the *venesectionist*) one of the four court physicians of Louis XIV., all of whom are caricatured under the thin disguise of Greek names carefully adapted to suggest to the audience of this particular play their most striking and ludicrous personal characteristics. "*M. Tomès.* Comment se porte son cocher.—*Lisette.* Fort bien. Il est mort.—*T.* Mort?—*L.* Oui.—*T.* Cela ne se peut.—*L.* Je ne sais pas, si cela se peut; mais je sais bien que cela est.—*T.* Il ne peut pas être mort, vous dis-je?—*L.* Et moi, je vous dis qu'il est mort et enterré.—*T.* Vous vous trompez.—*L.* Je l'ai vu.—*T.* Cela est impossible. Hippocrate dit que ces sortes de maladies ne se terminent qu'au quatorze où au vingt-et-un; et il n'y a que six jours, qu'il est tombé malade.—*L.* Hippocrate dira ce qu'il lui plaira; mais le cocher est mort." *L'Amour Médecin*, Act 2, Scene 2. Compare with this piece of admirable fooling the scene in *Le Mariage Forcé* (sc. 6), where the medical philosopher, Pancrace, overwhelms an unseen adversary with Aristotelian Billingsgate.

[1] *Medical Education, Character, and Conduct.* Introductory Addresses, delivered to Students of Medicine in Edinburgh and Glasgow, 1885, 1886, 1882. MacLehose and Sons, 1885, p. 63.

creature it was at least *possible* to represent a "physician" as having become in the days of Louis XIV., in the midst of a most brilliant outburst of literature and art, at the very time when Harvey's great discovery was slowly making its way against prejudices derived from the darkest of the Middle Ages, and the still overpowering authority of Aristotle and of Galen.

But at this time the Faculty of Medicine in Paris was probably the last retreat of obscurantism in all Europe, at least within the domain of the physician. In Italy, in England, in Switzerland, in Germany, in the Low Countries, the spirit of observation and experiment was awakening from a long sleep; and in many departments—anatomy, botany, physiology, surgery—things were moving on apace; but the physician was almost everywhere belated in the race. Even down to the last century, the man of *learning* (of the type of Linacre and Caius) in the Royal College of Physicians of London greatly predominated over the man of *science*, as exemplified in William Harvey; while Oxford and Cambridge, which alone could open the portals of the College, were absolutely *nowhere* as schools either of science or of medicine; and neither taught, nor professed to teach, anything but a mostly mediæval curriculum. And thus it came about, so late even as the year 1815, that the curious anomaly of the "double qualification" obtained a legislative sanction in English medical education. For while, in most European countries, the State and the Universities co-operated in arranging and controlling the issue of a single qualifying diploma for the general practitioner, the Royal College of Physicians of London was still too much the college of a learned *caste* to allow of their exclusive privileges being shared by any but University graduates; while the two great English Universities were altogether too helpless, as regards the necessary discipline of a medical career, to make it even possible for them to afford the slightest assistance. The Royal College of Surgeons, on the other hand, which had for ages concerned itself only with anatomy and surgery, to the exclusion of physic, continued on the even tenor of its way, including in its membership men ignorant of Latin, but instructed as regards fractures, dislocations, and surgical procedures generally; while the Worshipful Company of Apothecaries, cleverly perceiving and taking advantage of the enormous gap which was at once apparent between the technical discipline of the pure surgeon and that required for the all-round practice of the medical profession, marched into a position of legal independence through this gap, and, from being the humble servants of the physician, obtained for their licentiates not only the exclusive right to dispense medicines, but the status of prescribing them also, and thus a perfectly just and well-earned rivalry with the physician all over England. In Scotland we arranged matters somewhat differently; but the case of Scotland would imply a longer discussion than I can afford at present; and as I have already dealt with this contrast some years since, in another address,[1] I will pass it over now, at least in its political aspects. Fortunately, the sequel of the history, in so far as it dates from the Medical Act of 1858, is well known to most of us, and need not be here dwelt upon.

MODERN PHYSIC.—THE COLLEGES AND THE UNIVERSITIES.

What I am chiefly concerned to bring under your attention, however, in this connection is that, according to the historical development, or evolution, of medical education in this country, and especially in England, the physician, in the sense of *Naturæ studiosus,* the devotee of *φύσις,* as aforesaid, stood a very fair chance of being altogether, and finally, suppressed and wiped out of existence. Before Linacre, in 1518, obtained from Henry VIII. the letters patent which founded the Royal College of Physicians, we are told that the practice of physic was chiefly engrossed by illiterate monks and empirics; while the sole power of licensing practitioners of medicine lay with the bishops in their several dioceses; the survival of the power, as you know, remaining, up to a comparatively recent date, with the Archbishop of Canterbury in the Lambeth degrees. The charter of the College itself alludes to the "great multitude of ignorant persons, of whom the greater part had no insight into physic, nor in any other kind of learning"—who, nevertheless, rashly undertook and boldly practised the treatment of disease. But we may be nearly certain that Thomas Linacre, the pupil of Angelo Poliziano, the friend of Erasmus, the professor of Greek in Oxford, and the instructor in that language of Sir Thomas More, was not a particularly likely person to originate or patronise the kind of studies which, a full century later, shed a lustre on the college and on England in the person of William Harvey; and, although it is on record that Linacre, about this time, founded lectures on physic both in Oxford and Cambridge Universities, it is also very certain that the origin of anything like a medical school, involving the teaching of the art in a practical sense, was of much later date, and that, up to the present century, nothing like an efficient medical discipline existed in either university; so that the learned graduates who formed the ruling body in the English College of Physicians continued (no doubt with many notable exceptions) to be distinguished more as the inheritors of the Hippocratic tradition, than as the pioneers of a thorough and modern discipline, such as we now associate with the clinical and pathological study of our art. On the other hand, the very imperfect scientific training of the general practitioner, the early age at which he was often withdrawn from school, the distracting influence of the apprenticeship, and the tradesmanlike habits arising from the mode of his remuneration, all combined in keeping the L.S.A. upon a much lower professional level, and led to the survival in England of a kind of polypharmacy, which was, up to a quite recent date, nothing less than a discredit to the whole medical profession in this country. As Dr. Graves remarked, within the memory perhaps of some who still survive:—"The charge of inexperience is not necessarily confined to the beginner; it applies equally to many an old practitioner, whose errors have grown and increased in strength during a long series of years, because, from defects in his original education, from the absence of a properly directed clinical instruction, he commenced practice without having previously acquired the habit or power of accurate observation."

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[1] Introductory Address for 1882-83 (as cited above), pp. 10-13.

In an address which I had occasion to deliver to the students of this University, at the opening of the Session 1882-83 (*loc. cit.*, pp. 7, 17), I ventured to claim, on behalf of three of our great academic centres in Scotland, to have anticipated, in no inconsiderable degree, the Medical Act of 1858 in rectifying the more glaring of these defects in medical education. I concede at once that the Scotch graduate or licentiate was by no means an ideal "Doctour of Physike"; and that, according to the standard of the Royal College of Physicians, he was long very deficient in some of those refining influences which, drawn from a remote past, formed the character and moulded the career of such men as Linacre, Sir Thomas Browne, Sydenham, Mead, and Heberden. I concede also that he was for long, and perhaps still is, more or less wanting in the technical completeness of what is called the art of prescribing—the skill which enables a man to polypharmacize—if I may venture to coin a word, which I will further explain in terms of Voltaire's sarcastic phrase—neatly and profitably to put much physic and many complex mixtures, of which he knows little, into a body of which he knows less. But we claim for the Scotch graduate that, in the discipline of the art of medicine, he has, for several generations at least, been accustomed to make a good and a serious beginning by means of a regular academic curriculum, in which a foundation was laid for "physic," in the systematized study of certain departments of natural science. No doubt there were difficulties in the way, even of attempting so much, when in England the accepted type of medical discipline was that of the "surgery-boy"—the apothecary's apprentice for three years—finished off by one year in a London hospital.[1] But in the middle of last century, when William Cullen, and after him Joseph Black (the discoverer of latent heat), and after him Thomas C. Hope (the first investigator of the law of expansion of water according to temperature), became professors of chemistry, and then of physic, first in this University and afterwards in Edinburgh, it could not but be that a great impulse should be given to the idea that a physician was bound to learn at least *something* which could be dignified with the name of science, more or less exact, about the constitution of the world in which he lives, and the instruments with which he professes to work cures upon the human body. In point of fact, we know that such an impulse *was* communicated, and that in the Scottish Universities chemistry, as a science (and perhaps also botany), have never lost a distinctive place in the medical curriculum from that day to this. I wish I could say the same for physics proper—natural philosophy, as we call it—which has been, comparatively speaking, left out in the cold, or treated as a step-child. But in botany we have had at least one opening into the sciences of observation, which, though often depreciated by sciolists and persons calling themselves "practical men," has maintained its ground among us, and has had added to it more or less of zoology, of comparative anatomy, and of natural history in general. Again, I concede that these things have often been imperfectly done, at different times, in our Scottish Universities; but in the mere attempt to do them at all, and to fulfil a regular four years' curriculum with these as its basis, instead of the old, old wearisome variations upon the aphorisms of Hippocrates, and the controversies, ending in smoke, of vitalists, and solidists, and humoralists, I ask you to accredit the Scottish Universities with something like a new departure in the academic discipline of the physician.

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[1] See the description of this type, as still existing "five and twenty years ago," from an evidently English hand, in the *Times* of October 3rd, 1882; and my remarks upon it, reprinted at the end of this Address.

I might enlarge upon this topic, but that I am not really so anxious, upon the present occasion, to dilate upon what has been done in the past as to inquire what is still wanting to bring about the professional ideal of the physician's training. I happen to have been closely considering this subject lately, with a view to the education of a son of my own; and without attempting to justify my opinions in all respects in detail, I will, as it were, think aloud for your impartial consideration some of the thoughts that have been passing through my own mind, even if in a crude and undeveloped form.

WHAT IS STILL WANTING IN THE TRAINING OF THE PHYSICIAN.

From the earliest days of my experience as a teacher it has been customary with me to use expressions and to act in a spirit, of which you may readily judge for yourselves from the following brief paragraph, taken from an address to students of medicine, delivered more than twenty years ago:—

"The first lesson to be learned in order to make all other lessons possible is, in my opinion, this—to deal very largely with things and not with mere words; to realize as much as you can all your instruction by making it your own through personal observation; to suffer nothing, if it can possibly be avoided, to lie in the mind as a dead weight of vocables, oppressing the memory and dwarfing the intellect; but to bring everything into the living light of fact and of Nature, and thereby at once to assure to yourself the truth and exactness of your knowledge, while at the same time you are stamping it down upon the memory by the most sure and lasting of all technical methods."[1]

These words are not cited now as containing anything at all original or profound, but simply as an illustration of the spirit that, for a generation past or more, has ruled in the teaching of the Scottish universities. It is, in the very next line of the address, presented to the minds of our *alumni* of 1866 as "essentially the modern spirit of scientific inquiry, in virtue of which alone you can rise out of the dogmatisms and orthodoxies of the past, to make secure and beneficial progress in the knowledge of your profession." If you agree with me that these words are not in themselves inappropriate, nor yet extravagant or fallacious, as applied to our art, and that they come fitly from one who followed in the line of succession, as an academic teacher, to William Cullen and Joseph Black—then I will ask you to go with me one step further, and admit that during these hundred years past the Scottish universities have been, with more or less success, and amid considerable discouragements, labouring, on the whole, to build up again the great and noble ideal of the *physician* as *naturæ studiosus*, or Nature-seeker (Natur-Förscher) as the Germans call it; and this, in accordance at once with the most modern methods and the most ancient and venerable traditions of the Hippocratic age.

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[1] *Medical Education, Character and Conduct*, etc., (*ut supra*), p. 70.

No one can be more ready than I am to admit that there are, nay, that there must be, limitations in the very nature of the case to the too absolute recognition of this ideal. It is said by some that the spirit of modern science is ungenial and hard, even pitiless, and therefore not at all fitted for the ministrations of humanity; that it tends to make the suffering man, the *patient* (as we call him), into a mere *case*—a thing to be observed and noted rather than a man of like passions with ourselves, and therefore to be treated with consideration and sympathy. There may be just a grain of truth in this; and to whatever extent it is true, we of this medical school claim to be aware of the fact, and to be ever on our guard against the tendency. But none the less it may be affirmed with entire truth, and with cumulative evidence if need be, that all the evils inflicted on poor suffering humanity by the physician as scientist have been but a drop in the bucket as compared with those which have sprung from the too slavish adoption of traditions, in which there never was any trace of the scientific spirit at all. The physicians satirized by Molière were very surely far more open to the charge of professional callousness than any of those in the present day who have been remarked upon as presenting, even in their more extreme aspects, the tendencies of modern science. But it may be quite frankly conceded that when the physician, whether ancient or modern, has forgotten the claims of humanity, he is to be condemned alike, whether he proceeds along the lines of authority and scholastic tradition or according to the innovations of science and experiment. Moreover, it is certain that the day has long gone past when the physician, as healer, can expect for a moment to rival, or even come into competition with the scientist—the *physicus* in the etymological sense of the word—on his own ground. All that can be reasonably asked for him in these days is that he should be open to the influences, and should work in the spirit, and be subject to the corrections of the more exact sciences, in so far as they are applicable to the functions of the human body, whether in its sound or in its pathological condition.

I will venture in advance (although I know as yet but little more than the general subject on which he is to speak) to commend to your notice the forthcoming Address in Physiology by my esteemed colleague Professor M'Kendrick, as a sample of the kind of instruction which we of this university and medical school consider to be among the legitimate developments of the study of φύσις, as applied to our art. But in doing so I shall venture to anticipate, because I am well assured that anything I can say here will only give expression to what will still more clearly emerge from that discourse, some of the discouragements to which I referred a moment ago—the difficulties we experience in training our students adequately on the lines we have laid down for them. These difficulties are manifold, and they are not of our making, nor are they peculiar to Scotland. I have only time now to refer to two of them.

The first of these difficulties belongs to the medical curriculum, in which, although chemistry, botany, and natural history have long been with us a necessary part and, so far as they go, a well-conceived and valuable scientific basis for the more technical part of our teaching, nevertheless physics proper—or, as we call it, natural philosophy—the very first step in the study of the laws of matter—is still but very imperfectly recognized. This great omission has arisen, no doubt, from the fact that these laws were supposed to be taught, and in a measure were taught, in connection with chemistry, which, from its old hereditary relations with pharmacy and the pursuit of the philosopher's stone and the elixir of life, had from time immemorial a claim on the physician. But when we consider how completely modern science has demonstrated the subordination of living bodies and physiological processes, not to a wholly detached set of laws termed vital, but to all the most elementary laws of matter; and, further, the correlation of all the physical forces throughout the universe, so that the living body and its environment act and react on each other throughout infinite space and time, it will be readily admitted, I think, that some kind of systematized instruction in physics, and not a mere elementary examination in mechanics, should be an essential part of an education with a view to the medical profession. And when we further consider that most of the great advances in medical diagnosis in the present day, through the stethoscope, microscope, laryngoscope, ophthalmoscope, sphygmograph, electricity as applied to muscle and nerve, etc., involve applications of pure physics which are neither remote from practice nor yet very easily mastered by the beginner; and that, in the case of electricity and other physical reagents, even heat and cold, etc., we are every day extending the domain of these sciences in therapeutics, and still more perhaps in preventive medicine and sanitary science, their claim for an extended recognition in teaching seems to be enormously enhanced. I am persuaded that in a very few years the physical laboratory will become an absolutely essential preliminary step in the education of the physician of the future, and that those who have not undergone this training will be hopelessly distanced in the race.

But this leads me directly to the other difficulty, or disadvantage, under which the Scottish universities have hitherto laboured in endeavouring to restore to the healing art its ancient association with the study of Nature. And this is by far the graver difficulty of the two, inasmuch as its rectification depends in no degree upon us or upon any possible change in the medical curriculum, but upon what amounts to a practical readjustment of the entire edifice of general education.

WANT OF EARLY TRAINING IN PHYSICS AND NATURAL SCIENCE.

The evil to which I now refer, as some of you have already, no doubt, perceived, is the extremely unprepared state in which the minds of most boys and young men are found at the time of their leaving school, as regards the most elementary truths and methods of physical science, and of the observation of Nature. It is now more than a quarter of a century since this great defect in the English public schools attracted the attention of a Royal Commission appointed in 1861, and there is no reason to suppose that in Scotland at the time in question the state of school education was in this respect much better than in England. "Natural science," the English Commission reported, with some slight exceptions, "is practically excluded from the education of the higher classes in England. Education with us is, in this respect, narrower than it was three centuries ago, while science has prodigiously extended her empire, has explored immense tracts, divided them into provinces, and made them accessible to all."[1] About the same time that this Commission was appointed to inquire into the public schools, a most unfortunate result followed the application of the revised code of 1861, both as regards the training colleges and the elementary day schools. Evidence of the most convincing kind was given in 1872 before another Royal Commission to the effect that "the limitation of the examinations (under this revised code) to the subjects of reading, writing, and arithmetic unfortunately narrowed the instruction given in the elementary schools; and that this change, together with the lower standard adopted in the training and examination of pupil teachers, and the curtailment of the syllabus of the training colleges, exercised a prejudicial effect on the education of the country."

The recommendations of this last Commission,[2] presided over by the Duke of Devonshire, and commonly called by his name, were directed not only to the exposition of these evils in detail, but to their remedy; and it is not to be denied that some improvement as regards science teaching may have followed from these and other investigations conducted under public authority, and with great care and impartiality. Yet in 1885 a most important and instructive presidential address was delivered to the British Association in Aberdeen which, amid a great deal of brilliant and elaborate argument on the general subject of science as an element in the prosperity of nations, contained these words:—"The Commissions of 1861, 1864, 1868, and 1873 have expressed the strongest disapproval of the condition of our schools, and, so far as science is concerned, their state is much the same as when the Duke of Devonshire's Commission reported. . . . . No doubt there are exceptional cases and some brilliant examples of improvement since these words were written; but generally throughout the country teaching in science is a name rather than a reality. The Technical Commission which reported last year (1884) can only point to three schools in Great Britain in which science is fully and adequately taught. . . . . A return just issued, on the motion of Sir John Lubbock, shows a lamentable deficiency in science-teaching in a great proportion of the endowed schools. While twelve to sixteen hours per week are devoted to classics, two to three hours are considered ample for science in a large proportion of the schools. In Scotland there are only six schools in the return which give more than two hours to science weekly, while in many schools its teaching is wholly omitted. *Every other part of the Kingdom stands in a better*

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[1] *Report of the Public Schools Commission*, p. 32.

[2] *Second Report of the Royal Commission on Scientific Instruction and the Advancement of Science*, 1872, p. 9, section 8.

position than Scotland in relation to the science of its endowed schools. The old traditions of education stick as firmly to the school as a limpet does to a rock, though I do the limpet injustice, for it does make excursions to seek pastures new."[1]

It would have been extremely presumptuous in me to have asked your attention here to statements like these had they been made on my own authority, as the result of a necessarily limited personal experience; but when I add that these are the words of Sir Lyon Playfair, carefully considered, and advanced under all the sense of responsibility that can attach to a member of several governments, and from the Chair of the British Association only three years ago, it seems difficult to evade and impossible wholly to set aside the force of their appeal. I am well aware, indeed, that in Scotland, and even in our own city of Glasgow there are schools which have already made some considerable advances in the direction here indicated; and that in the High School of Glasgow, in particular, there exists now a chemical laboratory such as would do no discredit to any university. But, as regards the schools throughout the country, the advance has been so slow that, for a long time to come, our boys will leave their schools and our young men will continue to enter the universities in a state of great mental unfitness to grasp even the most elementary ideas of physical science, and therefore requiring more than ordinary care to ensure, at the very commencement of a medical education, the preparation in *physics* which will shortly become all-in-all to the true *physician.*

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[1] Address by the Right Hon. Sir Lyon Playfair, K.C.B., M.P., F.R.S., President. *Report of the Meeting of the British Association for the Advancement of Science*, Aberdeen, 1885, pp. 7, 8.

FARADAY'S EVIDENCE IN 1862.

Although I cannot here even attempt to discuss this matter as it deserves, I would fain hope that this mere allusion to it may induce some of my hearers to peruse some part of the evidence laid before the Devonshire Commission,[1] and even more, perhaps, that of a great number of eminent authorities who were examined at great length by the Public Schools Commission in 1862.[2] In particular I would direct attention to the words of one who has "built himself an everlasting name," not only as a profound and original investigator in physics but as the most attractive of teachers. It might well have been thought impossible that Michael Faraday, speaking with earnestness and conviction, though with the modesty of one who confesses himself at the outset to be "not an educated man," should be even as " a voice crying in the wilderness." Yet it was so. It appears in this evidence that in 1855, more than thirty years ago,[1] Faraday drew attention to the fact that the most highly educated minds in this country were often so entirely undisciplined in the merest elements (the A B C, as he expresses it) of the knowledge of *things* as opposed to *words* and *abstract ideas* that these minds in mature age remained impermeable to truths which to one with any scientific discipline at all were all but axiomatic. The " want of judgment in natural things " (as he calls it), the " great deficiency in the power of giving the reason why "—were such among the educated class as often to defeat the ends of justice in courts of law, and to lead to the most humiliating exposures of confident ignorance and presumption in matters of fact; and all this in the case of persons who had received every advantage, and who, in all kinds of learning and in the discipline then provided in the schools, stood in the front rank of Englishmen. And this deplorable result —this all but entire omission to educate one side of our many-sided human nature—was as gratuitous and wilful as it was deplorable. For Faraday had abundantly satisfied himself, through his personal experience as a teacher in the Royal Institution, that these elementary ideas, this primary cultivation of the "judgment in natural things," was a discipline not only easily conveyed into the minds of children, but even having a quite peculiar fascination for them. "The young mind," he says, "as I find it . . . is very observant and asks most acute questions. I do not find it, generally speaking, backward to understand a statement I make in simple language; and if I tell him this or that . . . and then shape it into a question, he can generally answer me. I must confess to you that I find the grown-up mind coming back to me with the same questions over and over again. . . . Their minds are not prepared to receive or to embody these notions, and that is where you want education; to teach them the A B C of these things. . . . I never yet found a boy" (he says in another place) "who was not able to understand by simple explanation and to enjoy the point of an experiment."

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[1] *Sixth Report of the Royal Commission on Scientific Instruction*, 1875 (dealing with the teaching of Science in Public and Endowed Schools).

[2] *Report of H.M. Commissioners appointed to Inquire into the Revenues and Management of certain Colleges and Schools, etc., with an appendix and Evidence*, vol. 3; Evidence, Part 2; General Evidence, p. 363, including that of Dr. W. B. Carpenter, Sir Charles Lyell, Faraday, Hooker, Owen, and others.

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[1] In a lecture delivered at the Royal Institution, in presence of Prince Albert, and handed in to the Commissioners on Public Schools: Evidence (as above), p. 376.

I am almost sorry that I cannot here trespass more largely on your time in quoting samples of this most remarkable evidence; but I will only detain you one moment longer over it in order to ask, Is it not most strange that we are able, many of us, to hear these things in 1888 as if they were a perfectly new and fresh revelation of the capacities for education, in a scientific sense, of the minds of our children? How many of our own boys and girls, in the best schools that we have been able to send them to, have been in the way of securing even a moderate amount of the *kind* of discipline—the A B C of science here alluded to? For, observe, Faraday makes it an absolute condition of the training he proposes that it shall be *real* of its kind—that is, *experimental* and not *doctrinal.* And here arises the great difficulty—where are you to find the teachers? "You want men who can teach," he observes, "and that class has to be created." Again, "Lectures depend entirely for their value upon the manner in which they are given. It is not the matter, it is not the subject so much as the man." And yet again, "Everything in these matters must depend upon the spirit and the manner in which the instruction itself is conveyed and honoured. If you teach scientific knowledge without honouring scientific knowledge as it is applied, and those who are there to convey it, you do more harm than good. You only discredit the study and the parties concerned in it."

Faraday was perfectly well aware of the difficulties of detail that his views would have to encounter—the obstruction arising from the incompetency of the average schoolmaster[1] even to take in the idea of the importance of elementary science as a discipline when properly taught, and to assign it, accordingly, a fair amount of time in the scheme of education. Although "not an educated man" (according to his own statement), he was a clear-sighted one, even where he had no personal experience. "I should be very sorry," he says, in answer to Sir Stafford Northcote, "that the introduction of science should be an injury to any other branch of knowledge. I say it in those general terms because I have not experience enough to know what the effect will be. As a general impression, ... I think that one fifth certainly of the time which an individual devotes to study should be devoted to the attainment of natural knowledge. In what shape you should give it I cannot say. I think that in less than half a century it will deserve and obtain far more, but I am not for doing hastily an injury to the schools as they

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[1] On this point his words are definite enough, though I have refrained from inserting them in the text. "I am often asked,"

he says in answer to Question 22, "what men to recommend to spread a knowledge of natural physical things. I cannot recommend such; such men hardly exist at present. They want the A B C of science; not the X Y Z; they want the first elements. If I give a juvenile lecture, I lecture in as plain terms as I can to children, and I simplify as far as I can the ideas; *but men are not educated as yet to do that: the ordinary schoolmaster does not know how.*" And in another place he says of the ordinary school discipline, "It does not blunt the mind, but it so far gives the growing mind a certain habit, a certain *willingness to accept general ideas of a literary kind, and to say that all the rest is nonsense*. . . . . It takes up the general impression that a *certain kind of knowledge, what I call the real knowledge, the knowledge of things, is of no importance.*" Qu. 30, 31.

are at present established. Nevertheless, I beg you to understand that I am a very imperfect judge on these matters."

I venture to place before you these words from an almost forgotten Blue Book, because they seem to me as fresh as ever in their lesson to us now, as regards the training of the physician of the future. Moreover, by the very modest as well as the true wisdom inherent in them, they convey an implied rebuke to the arrogance and all-sufficiency of science in its own eyes, as much as to the traditional dogmatism and quasi-authority of the schools as then existing. Moreover, they afford a cover for one who is no Faraday, but who has a very abiding sense of the importance of Faraday's view of the position, for pleading in behalf of this before the British Medical Association. Personally, I have long held the view that no child is too young to take in what this consummate master of science calls the A B C, that is the methods and some of the simpler processes of science; the manner *how* and the reason *why*, as Kingsley puts it; and in the ever-expanding cycle of the human faculties, from infancy upwards, it is quite curious to see how these, if they are not artificially interfered with and stunted, work into a spontaneous and (I believe) wisely-ordered harmony with all that is best in the linguistic and more commonly received scholastic elements of education, so that, far from suppressing, each of them protects, supports, and even nourishes the other. But our present programme of school discipline is still too largely founded on the idea that at six or at seven years old, as the case may be, a child may be withdrawn, in a great measure, from this strictly natural, wholesome, and all-round education of the faculties, to have his nose buried for ten long years in books and papers and mental and other arithmetic, till his little eyes become myopic, and his little brain suffers a corresponding deformity. Athletic sports, as a counterpoise, are no doubt good, and industrial training better, in some cases, especially for the children of the poor; but they do not supply the "missing link," which can only be found in the early discipline of φύσις, pursued in the very spirit of Faraday. And here it is, therefore, that I think the physician, by returning in some measure upon the old, the oldest, lines of his professional discipline, may establish a strong claim to be heard, even in the matter of early scholastic training, as against the too narrow view of the professed educationist. He will at least have as strong a claim as the old Scotchwoman, who (no doubt in the interest of φύσις, though possibly she knew it not) is reputed to have ventilated her dissatisfaction with the received methods in the education (so-called) of one of her boys at the parish school in these pithy, if not sapient, words: "Sin' ever he gaed to the schule, his edication's been stopit a' thegither!"

RELIGIO MEDICI.

I have now to ask your attention, for a very few minutes only, to a concluding topic which I approach, indeed, not without fear and trembling, but which is of too much prominence and importance in itself, in connection with the subject of this address, to allow of my passing it by without some reference.

Probably there may be some of you here present who have been led to take note of a proverb which I am bound to say I have not been able to trace to its source, but which I suspect to have been the growth of that mediæval period to which allusion has already been made in the course of this address: "*Ubi tres medici, duo athei.*" I am not concerned in tracing out for you, even if I were able to do so, the most probable origins of this defamatory saying; nor shall I spend many words in venting my honest indignation upon it as a calumny and a reproach. It will be wiser and more profitable in every way to take it as it stands—as an example of what the late Earl Russell said of proverbs in general—"the wisdom (or in some cases the foolishness) of many," accentuated and condensed into a telling phrase by "the wit of one." From this point of view, it may be that there is something more or less worthy of careful reflection in this proverb, even if we should disown it in its literal acceptation. But I need scarcely say to those who are at all conversant with philosophical studies, that to have been accused of atheism in the Middle Ages, may be quite the reverse of a real reproach to any man or set of men. From the time of Socrates downwards, indeed, this reproach has been a part of the stock in trade of vindictive, even if sincere, ignorance and bigotry all over the world; and to have been tabooed with atheism is often, almost without qualification, a passport into the ranks of those who have kept alive the flame of the human spirit, tending, and often vainly struggling upwards, to escape from the jargon of scholastic controversies, and the mephitic vapours of ecclesiastical strife. From this point of view it was inevitable, nay it was essential, that the physician or naturalist, in so far as he really was, or aimed at becoming, such, should incur this reproach. The marvel rather is, to us of this 19th century, that those who incurred it should have done so little to deserve it. The reproach, too, from a philosophical point of view inconsistent with atheism, but not seldom conjoined with it, of tampering in an evil sense with magic, was sure to be launched, in those days, at men who professed to be successfully investigating the "*secreta naturæ*" by others than the most orthodox methods. And the prosecution and imprisonment of Roger Bacon, on the one side, and of Galileo on the other, not to speak of the numerous "martyrs of science" both before and after these, will remain as an imperishable record of the blind and impracticable spirit of mediæval dogmatism, which, covering itself with the mantle of religion, stood athwart the path of the physician for hundreds of years. But although the condemnation on the side of godlessness came most easily and naturally out of the mouths of ecclesiastics, it is not by any means to be inferred that, either in ancient or modern times, it has been confined to them. Even in the kindly and thoroughly human word-picture, drawn for us by Chaucer, of the typical "Doctour of Physike," in the prologue to the *Canterbury Tales,* (line 440) it comes out that while

"Wel knew he the old Esculapius
And Dioscorides and eke Rufus,
Old Hippocras, Hali, and Gallien,
Serapion, Rasis, and Avicen,
Averrois, Damascene, and Constantin,
Bernard, and Gatisden, and Gilbertin,"

yet, alas! as a sad balance in the way of defect to all this learning of ancient, and also of then quite modern, date, it is quaintly and humorously added in a single line, without even an attempt at amplification or excuse, that

His studie was but little on the Bible.

I had the curiosity to submit this passage to the judgment of two of the most noted students of early English literature—one of them perhaps the best living authority on Chaucer—that I know of in these islands, with the remark that in an age when the study of the Bible by lay persons is supposed to have been discouraged, if not proscribed, by the Church, it is curious, to say the least, to find this made a matter of reproach, or even of satirical reflection, at all. The replies of both were alike instructive and quite in accord; but I give you *verbatim* that of the more professed Chaucerian, as being both brief and exactly to the point.

"Chaucer, I fear," he writes, "was not altogether above popular prejudices, and among those to which he fell a victim was one which is as venerable in age as it is enduring, and to which the Emperor Napoleon gave expression during his last days, when he said of his physicians, '*Ces messieurs qui ne croient à rien.*' Chaucer's Doctor of Physic was very respectable and very well read, but he was something of a materialist. It is, I think, a mistake to suppose that in Chaucer's time the Bible was a closed book to the laity in general. Even before Wiclif there were at least paraphrases in plenty. As to the learned, they certainly studied it, and the satire of Chaucer against the Doctor of Physic was that, while he took kindly to all sorts of learned books, he instinctively turned a cold shoulder to one where he would find something besides learning." [1]

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[1] My other correspondent writes, *inter alia*, as follows:—"The like was said of the Arabian physicians, who, with the Greeks, were, up to and after Chaucer's time, the chief sources of medical opinion. Averroes cared more for Aristotle than for Mahomet,

This, then, is the position and the stigma that we have to deal with as physicians or students of Nature and science in the present as in all former ages, in proof of which you will allow me simply to refer (without at all dwelling upon it) to an article in the actually current number of the *Contemporary Review.*[1] I will, therefore, close this address with a very few of my own thoughts on the subject, not at all in the way of controversy or of recrimination, but as expressing the matured convictions of a lifetime on a theme which must needs come home to every man's conscience in the exercise of our profession, and on which I should despise myself if either the desire of saying smooth things, or the fear of saying the opposite, were to move me in the least degree in addressing an assembly like this.

That the active ministry of the *healer*, if fitly and

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and was said to have expressed the wish for his soul—"*Moriatur anima mea morte philosophorum.*" He is said to have called Christianity an impossible religion, Judaism the religion of children, and Mahomedanism the religion of pigs. Averroes died about 1216, but the Arabians before him bore the like character for putting science in the place of religion. When the studies of the Saracens were influencing in many departments of thought the best minds in Europe, the physicians among us based, necessarily, their science on the teaching of the Greeks and Arabs, which would give them speculations upon Nature that must often have been out of accord with mediæval orthodoxy."

[1] "The Scientific Spirit of the Age," by Frances Power Cobbe. *Contemporary Review*, July, 1888, p. 126.

diligently pursued in a serious and not a sordid spirit, cannot possibly tend to irreverence, or to what I would call essential atheism or godlessness, is, I think, so obvious that it is only wonderful that any doubt should ever have arisen on the subject. That ministry is the ministry of the body, no doubt; physical, therefore, in its aim; physical also, to a certain extent, in its limitations; and I am not one of those who would argue that, because it is so, the physician is thereby degraded and straitened unless he is also constantly invading the province of the religious teacher. But when we consider how closely the one province trenches on the other; and further, how inextricably both mind and body are involved in all the greater and graver crises of the lot of man on this earth—birth and death, sickness and health, moral contamination producing disease, and, on the other hand, physical disease inducing moral aberrations, and, with or without these, positive insanity—we must acknowledge that the spiritual element in man is brought, necessarily, into the sphere of the physician's daily work. I am confident there is not a man in this room who will not emphatically agree with me in saying that a physician who even inclines towards irreverence as his habitual attitude of mind is thereby disqualified from performing aright the greatest of all the services that at times he can render to the sick. I am not saying—because I do not know—how it may have been in times such as the age of Chaucer, when the iron tyranny of the Church made independent and personal convictions often impossible to a physician conversant chiefly with his art; or in the age of Napoleon, when the disintegrating and revolutionary forces which still prevail largely in France were almost at their maximum of destructiveness; but I do say that in this our own time and country, when the thoughts of men, however distracted in details, are seriously bent upon the greatest of all questions involving both time and eternity, the practising physician who can treat these questions with unseemly levity, or who even stands aside and says that they are out of his reckoning altogether, is an anomaly, and, to that extent, a man out of his place in our social system and atmosphere. But I desire to qualify this statement with another, which may or may not appear to some of you to be opposed to it, that in the case of the man of pure science, or the absolutely devoted naturalist, I can easily recognize even in the most advanced agnostic (in his own estimation), one who in the confusions of the present time is really pervaded by what I make bold to call a Divine spirit; one who in the search after truth and fact (as they are in Nature) is so regardful of these and these alone that he refuses to commit himself to positions in abstract thought that may seem to himself, or to others, to carry implications opposed to the perfect impartiality of his research.

I will not here name living men, to whom, in my opinion, or (I would almost say) according to my knowledge of them, this description is fairly applicable. But in the case of one great man, at least, who has lately passed away from our human and temporary judgment, we have been favoured with the most ample materials for seeing into the innermost convictions of a very noble character; and I say here, without a moment's doubt or hesitation, that a man who will, in the face of this evidence, call Charles Darwin a merely atheistic, an irreligious, or an irreverent man, pronounces thereby a most severe criticism on religion itself, and an implied condemnation on half the saints in the calendar, to whichever of the churches he may pin his faith. For in Charles Darwin (however we may explain it) we have, if his character is carefully and charitably studied, a man of the very stuff and moral fibre of which the most eminent saints are made. Grant, if you please, that he fell away from the conception of God as entertained by you, and to that extent came short of your ideal of humanity and of what you call saintship; no one was more sensible of this, or more candid in confessing it, than he was himself, when he described a large part of his own higher nature as having become (as he said) "atrophied" in the life-long course of developing his other faculties, which he almost with a grim humour (as one might say) described as those of "a machine for grinding large general laws out of large collections of facts."[1]

But, while you learn to use the lessons, grave as they undoubtedly are, of this magnificently candid avowal on Darwin's part, do not close your eyes to the life-long and unswerving devotion, amid pain and physical disabilities, to the work that was given him to do; the constancy, the transparent simplicity of character, the courtesy under differences of opinion, the chivalrous self-repression in argument, the consummate sense of justice, the abiding conviction that truth (shall we not say for him, God's truth) stands far and away above the level of human passions and infirmities in expressing or defending it; above all, take note of the abounding, the almost inexhaustible charity (in the highest Christian sense of the word) of that sweetly-composed nature, whether as shown in his published works, in his correspondence, or in the sacredness of the domestic circle; and then say for yourselves in what hierarchy of canonized saints you will find many that are his superiors, or even his equals, in all of these eminently Christian virtues and graces taken together. May we not say, then, that the life and work of Charles Darwin are a permanent lesson to all of us, whatever we may think of his theories and his confessedly somewhat agnostic position, not to restrict too much our own conceptions of what may or may not enter into the religious idea of a man's life here, but to leave it in all humility to one who looks on the various phases of man's aspirations " with larger, other eyes than ours," and to whom it is at least possible, if not probable, that in this our day (as in more ancient days) the current readings of the religious character may be sometimes quite changed from ours, when in the Kingdom of God the " first shall be last, and the last first."

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[1] *Darwin's Life*, vol. i. p. 101. Compare p. 311, where he speaks of his early emotions in the presence of grand scenery as having completely died out, so that "it may be truly said I am like a man who has become colour-blind."

I have not made this allusion without a definite purpose, though feeling that I am on dangerous ground, on which there may be some reasonable hesitation in admitting that one who has intellectually lost hold of some of the most central truths of religion, may nevertheless retain a more than ordinary share of its innermost spirit. But, if (as I am most ready to admit) the life and work of the great modern apostle of evolution tend towards the belief that science in these days, and especially the study of natural phenomena in the organic world, make, on the whole, rather for the weakening and dissolution than for the building up of religion, then it will assuredly be the part of the physician of the future to act as the mediator between the destructive and the constructive influences. Although he can never again become what he was in the early ages, the sole or the chief representative of physical science, he must always be, and must become more and more, a man trained in its discipline and familiar with its resources; while, on the other hand, his close relations with suffering humanity, and with the awful and solemnizing ministrations of life and death, will serve to keep him in a region apart from that of pure science, and one in which, from day to day, the voices from the unseen world (if he will only listen) are ever sounding close to his ears. It is not for me, here and now, to show how this work of conciliation is likely to be undertaken or to be accomplished, but that it must needs be undertaken, and will ultimately be accomplished no one can, I think, doubt who has not allowed himself to become, as Darwin puts it, "colour-blind" to the higher spiritual influences. And in this work of conciliation the Bible will assuredly hold its own as the great spiritual guide of humanity, and as an inexhaustible treasury of spiritual wisdom, if it be studied according to the meaning of its own simple words, and not according to the glosses that have been passed upon these by innumerable generations of contending theologians.

The physician of the future will, I believe, be much more, instead of less, inclined to make his study of the Bible than hitherto, and in this respect will differ greatly from the representative and typical "Doctour of Physike" of the *Canterbury Tales.* But he will study it in the spirit of modern scientific freedom and of historical research, not under the influence of mere tradition and ecclesiastical authority. And thus only, as it seems to me, can the reconciliation of science and religion ever be brought about.

In the address I had the honour to deliver last year in Dublin, I reproduced a sentence from one more than twenty years old, in which I had said as regards our own historical position at that time (but only in reference to the healing art), "The day of orthodoxies is over; the day of real science is only just dawning." The whole course of that address, as well as of the one which preceded it, was directed towards the demonstration of the enormous evils that have accrued to humanity and to the medical art from a blind reliance upon the tradition of the ages, and often upon traditions wrongly interpreted. "It is so hard," writes our humorous and venerable *confrère* across the Atlantic, Oliver Wendell Holmes,[1] "to get anything out of the dead hand of medical tradition. The mortmain of theorists extinct in science clings as close as that of ecclesiastics defunct in law." The abuses of blood-letting, of a senseless and obstructive polypharmacy, and of innumerable so-called remedies, either inert or positively mischievous, which have had to be cleared out of the way before medical science and practice could even begin to be reasonably simple and intelligible, have been a lesson to all of us, I trust, as to this "dead hand of medical tradition." How can we, of all men, fail to see that the same kind of mortmain, precisely the same or similar dead scholastic orthodoxies, have locked up the living words of the New Testament, and of our Lord Jesus Christ himself, in formulas around which the dust of ages has accumulated, till the dead letter has well-nigh choked out the living and life-giving spirit of Christianity. The beautiful story of a Divine life and death, the miracles of healing, the sermon on the mount, the parable of the prodigal son, with its ever fresh lesson of the fatherhood of God; that of the good Samaritan, with its large sense of human brotherhood, are conspicuous by their absence as principles from those creeds of the Church which profess to be the formulated essence of Christian doctrine; and instead of these we have the controversies of the first four centuries, marred and scarred all over with the traces of very human, very bitter, and often sanguinary conflicts, the scrambles for power of this or that bishop, lording it over the heritage in opposition to the very words of St. Peter, and giving to all time an example not of the "peace and goodwill" with which the first Christmas day was ushered in, but rather of the sad prophecy "not peace, but a sword," which only too truly foreshadowed the ecclesiastical as opposed to the Christian *régime.*[1]

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[1] *Currents and Counter-Currents in Medical Science; with other Addresses and Essays.* By Oliver W. Holmes. Boston, 1861, p. 22.

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[1] On the Apostles' Creed (so-called). See *Histoire du Credo.*

The physician of the future will do well if he remembers always the pernicious despotism which has been exercised over his own art (though in a minor degree) by the fetters of these dead orthodoxies, and will therefore be very slow to acknowledge their claims upon him to any more than a historical regard, even in the realm of theology. He will say of them in the noble words of the Westminster Confession, which (but for the formula connected with it in our Scottish churches) might almost be taken as the magna charta of Christian liberty in all such documents:—"All synods and councils since the Apostles' times, whether general or particular, may err, and many have erred; therefore they are not to be made the rule of faith or practice, but to be used as an help in both."[1] But, I desire you very specially to remark as my own personal anticipation, shared, I have no doubt, by many of those now present,—the physician, in his character of student of Nature, will make, and in the end will estab-

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par Athanase Coquerel, fils, esp. p. 20.—On the Nicene Creed, Milman, *History of Christianity*, Vol. iii., Index; *History of Latin Christianity*, vol. viii. p. 391. The creed commonly called of St. Athanasius speaks for itself, and requires no commentary. On the Westminster Confession many most interesting personal details will be found in the *Letters and Journals of Robert Baillie*, A.M., Principal of the University of Glasgow, MDCXXXVII, MDCLXII. 3 vols. Edinburgh, 1841.

[1] *The Confession of Faith*, agreed upon by the Assembly of Divines at Westminster, etc., chap. xxxi. Of Synods and Councils, 1643.

lish, this claim to emancipation, not in virtue of any irreverent, much less atheistic, tendencies, but for the very reason that he has access to a revelation of God, distinct from the written revelation, and requiring a wholly distinct method of investigation. In obedience to this call he will, sooner or later, absolutely decline to walk in the leading-strings of ecclesiastical tradition. And in so doing he will (far from fulfilling old Dan Chaucer's satirical description) studiously insist upon the Bible, and especially the New Testament, and above all, the recorded life, works, and words, of our Lord himself, as containing, by implication, the charter of his emancipation, and the only perfectly free religious atmosphere as yet opened to human thought and inquiry. In proof of which I will now only submit one pregnant saying, with which, if it be indeed the word of God, all those who believe it to be such are bound to find all the other words of God in entire accord:—

"Henceforth I call you not bond-servants; for the bond-servant knoweth not what his lord doeth: but I have called you friends; for all things that I have heard of my Father I have made known unto you."—John xv. 15.

APPENDIX TO CHAPTER I.

EXTRACT FROM INTRODUCTORY ADDRESS DELIVERED IN THE UNIVERSITY OF GLASGOW, TO THE STUDENTS OF MEDICINE OF THE SESSION 1882-83.

I AM reminded that only the other day we had set before us, in the shape of a leader in *The Times*,[1] and in connection with the opening this month of the various medical schools in London, a singularly clear and luminous appreciation of the changes introduced into medical study and practice in England, as the consequence of the Medical Act of 1858; from which anonymous, but not unauthoritative, statement it may be admissible to borrow a few sentences as an illustration of our present position; keeping in view always that the writer of the article in question, well-informed and thoroughly impartial as regards his own division of the United Kingdom, has had England, and apparently England alone, in view, in describing the state of medical education before 1858, and the changes gradually effected, owing to the legislation of that year. Were one at liberty, indeed, to guess (for every one knows we can do no more) at the authorship of that particular article; to see with the mind's eye the Vulcan that forged the thunderbolt of Jupiter on that particular morning, I should be disposed to say that, judging from internal evidence alone, the brain that guided the hand that wrote that particular article was also one of the most active of those recently at work in elaborating the Report of a Royal Commission appointed to inquire into the Medical Acts, with a view to their consolidation and amendment. If this be so, it gives, for us, an additional interest to the sentences of that article which I shall presently lay before you; and although it is by no means my intention to make this address in any respect a political or controversial one, it is certainly a fact to be noted that an Englishman, presumably well acquainted with the facts, and speaking of England and for England alone in the columns of *The Times*, should employ the following expressions as regards the state of medical education a year or two before the passing of the Medical Act of 1858, and yet a year after my first Introductory Address to Medical Students in 1856:—

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[1] Extract from *Times* of October 3rd, 1882.

"Five-and-twenty years ago, except in the case of the small number of men who graduated in medicine at Oxford or Cambridge with a view to consulting practice as physicians, the education of a medical student commenced by an apprenticeship to a licentiate of the Apothecaries' Company, which was then the chief source of the medical qualifications of general practitioners. The apprenticeship was rendered necessary by the Act of 1815, from which the Company derived its powers, and it usually commenced at about the age of fourteen years, so as to break prematurely into school education. The first year of the apprenticeship was often spent in the work of a surgery boy, varied by the preparation of medicines, and by the attainment of some knowledge of their properties. The later years were devoted to learning the business of conducting a medical practice, the art of talking to patients, and so forth; and, when the apprenticeship was concluded, a short term of hospital study formed a prelude to the single examination which, when passed successfully, permitted the candidate to work his will upon the sick under the protection of the law. Originally, the full term of five years apprenticeship was served in the house of the master; but as the term of hospital study[1] was gradually extended from one year to nearly three, the two demands were found to consume an inordinate amount of time, and the last two years of apprenticeship were commonly remitted, and were suffered to form part of the period of hospital study. Even then the total period of professional as distinguished from general education extended over about six years; and, as the examination might be passed at the age of twenty-one, was usually commenced not later than fifteen."

I interrupt the quotation at this point simply to say that one has to go back a long way in the history of medical education in Scotland to find a state of matters at all resembling the one here described. We had our errors and deficiencies, no doubt, and to some of them I shall allude more or less distinctly before I have done; but at no time within my recollection, nor, I think, within the recollection of those who preceded me, and whom I remember as my own instructors, was it even possible in Scotland to mistake a "surgery boy" of fourteen, prematurely withdrawn from school in order to manufacture pills and potions, and run errands, and make himself generally useful in a "shop" where he was under indentures to an "apothecary," for a regular student of medicine. To whatever extent the apprenticeship system held its ground in Scotland twenty-five or even fifty years ago (and far be it from me to say that in the hands of such men as Abercrombie, and others like him, it was not a system capable of much good) it was always controlled by a regular academic discipline, emanating mainly from the Universities, but largely reinforced, I am bound to say, by the co-ordinate influence, and often rivalry, of some of the corporations in building up what is now called the medical curriculum. In Scotland, at the period here referred to, a medical student, so-called, was rarely less than seventeen, or, at the very earliest, sixteen years of age; and he was bound at the very outset to constitute an "*Annus Medicus*," *i.e.*, to engage in an amount and kind of real study which effectually precluded the possibility of his being allowed to run wild as a "surgery boy," such as is alluded to in the *Times*' article; a type from which the "Bob Sawyer" and "Ben Allen" of Dickens' well-known story was only a too natural outgrowth. I venture to claim for the Scottish medical student of twenty-five years ago, even while admitting many deficiencies in his education both general and professional, that he was (saving a few sad exceptions) for the most part not only bound over to learn, but really in earnest in learning, the scientific as well as the practical work of his profession; that he was not devoting days or years which should have been occupied by ordinary school training, to "the preparation of medicines, and the attainment" (by chance) "of some knowledge of their properties;" but that he was, as I hope and believe you are, seriously impressed with the need of really scientific training as a foundation, and actually carrying out such training in anatomy and chemistry, even during his first year. Moreover, it lies strictly within my personal experience in 1856, when I had already taught several classes of Practice of Medicine, some of them numbering more than 120, to declare that the type of medical student existing at that time in Scotland was one which it was quite easy and natural for me to address as an assembly, not of undisciplined schoolboys out of school, but as "gentlemen" preparing, not without the solemnity and gravity proper to men, for the great business of life. I will add that this has been my uniform experience of medical students, as a class, from first to last; and I appeal to the words of both my introductory addresses, in proof of the fact. I affirm that it would have been practically impossible to use the words of these two addresses, seriously and sincerely, to a body of men in whom the "Bob Sawyer" element formed any considerable intermixture; and I claim for our Scottish system of University and other regular medical education whatever credit may accrue to it from that fact.

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[1] To a reader unaccustomed to the phraseology here employed, it may be necessary to explain that "hospital study" throughout this quotation corresponds to what in Scotland would be called regular medical study within the curriculum, whether in hospital or not. In the London schools, which are all attached to hospitals, this is well understood, but not so in Scotland.

I now return to the quotation from *The Times;* but what remains of it, though most important to us, is very short:—

"The Medical Act of 1858 introduced changes by which apprenticeship was abolished and the hospital course extended to four years, so that up to the age of seventeen school education might be continued. In order that the opportunity thus given might not be neglected, the student was required to pass an examination in general knowledge before he was permitted to commence his hospital career, and two professional examinations were instituted in place of one. The general result of the change was to occasion a marked advance in the intellectual status of students and of young practitioners. They lost, it is true, the familiarity with the management of a practice which apprenticeship was calculated to afford, and they lost opportunities of seeing many common forms of disease which are hardly severe enough to enter hospitals, but they gained more than an equivalent for these losses in their better education and their greater self respect. They gained an advance in intellectual training which rendered them better able than their predecessors to grapple with the complicated problems so often presented by disease."

It thus appears that the changes introduced by the Medical Act of 1858 into English medical education were in a great measure those which had been in operation in Scotland long before, under the influence of our three teaching Universities, and the medical schools associated with them in Edinburgh, Glasgow, and Aberdeen. Not that I wish you to infer that our state was all that could be desired, or even all that, under more favourable conditions of medical legislation, it might have become. The Apothecaries' Company, which I by no means wish to disparage, inasmuch as it has honourably striven to fill a position left to it by the other corporations, and thus to maintain the organization of the medical profession in England, long enjoyed a practical monopoly for its licentiates of most of the offices in that division of the kingdom which formed the usual introduction to general practice. The Royal College of Physicians during the same period long declined to admit, even as licentiates, any but the graduates of Oxford or Cambridge; thus aiming at a degree and kind of exclusiveness which effectually barred all possibility of its becoming the college of the medical profession. The Royal College of Surgeons, with a much more generally accessible, and therefore more popular, diploma, confined its whole attention to anatomy and surgery, under the theory that the other branches of a complete medical education were the province of the sister college, or of the apothecaries. Hence the anomaly of the "double qualification" which distinguishes England from almost every civilized country in Europe, for it involves the still more serious anomaly of the "single," or, as it may justly be called, the *half*-qualification, in virtue of which a man may enter into practice capable of performing operations, but entirely ignorant of medicines, and the diseases requiring their use; or, *vice versa*, capable of dispensing medicines, but almost entirely ignorant of anatomy and of surgical operations; and in either case far too much of a mere craftsman, far too little of an educated professional man.

The Scottish Universities early perceived this anomaly; and long before the Medical Act of 1858 was passed, a regular curriculum of study had been instituted which was unquestionably (though still defective in many points) far more complete than anything hitherto attainable in Great Britain as a qualification for general practice. Not only so, but by a preliminary training in what we should now call the elementary biological sciences, botany, natural history, chemistry, comparative anatomy, and physiology, the mind of the student was opened to more than merely utilitarian views of his profession; the "surgery-boy" type, to which I have already alluded, was effectually eliminated, and replaced by a far higher kind of professional culture, founded upon a broad and generous estimate of the utmost that was then held possible between the ages of 17 and 21, to fit a man for the practice of every department of a profession which requires, and fully engages, all the energies of a well-disciplined intellect. It is true that in respect of what may be called scholastic training the Scottish graduates were often found deficient; the trifling modicum of Latin which, in my time, represented the whole of the non-professional culture required being a very poor substitute for the B.A. degree of Oxford and Cambridge, or any of the other insignia of the ideal academic man. True, also, that on the other side it was sometimes objected that the Scottish graduate in medicine was not sufficiently conversant with the details of compounding and dispensing powders, and pills, and mixtures, and above all draughts (at 2s. 6d. a piece) to be taken two, three, four, or five times a day; in other words, that he had not sufficiently mastered the technical details by which his neighbour, the English apothecary, was able to accomplish the great ideal of the developed "surgery-boy" type—the dispensing of immense quantities of "physic" in the most complicated prescriptions, to pass unquestioned down the willing throats of Her Majesty's lieges. But in spite of these two grave deficiencies, and perhaps many more in detail of a more real character than either of them, the Scottish University medical graduate has struggled on, with every precedent and every national prejudice against him, to a position of great credit and influence in the sister kingdom; and what is far better than that, to a more and more elevated conception of his art, and the sciences on which it rests. The cause of this gradual elevation of his ideal is that the Universities, which did not wait for the Medical Act of 1858 to make a beginning were, nevertheless, fully prepared to advance with the times, and carry out as rapidly as possible all the improvements suggested by that Act and by the Medical Council which it created. A Royal Commission, appointed in 1858 in accordance with the Universities (Scotland) Act, enabled them greatly to improve their internal organization, just at the time when the Medical Act was initiating the revolution in English medical education and practice to which I have already referred. And now we are able to cite the very words of another Royal Commission, to show that our labours have not been in vain. "We readily acknowledge," write the Commissioners on the Medical

Acts in 1882, "all that the Scottish Universities have done for medical teaching and examination, and we should hesitate to make any recommendation which, in our belief, would interfere with their usefulness or prosperity. We do not propose to interfere in any way with their teaching, nor do we believe that students will cease to recognize the cheapness and excellence of their teaching." These, gentlemen, are not the words of Scotsmen, or of Scottish graduates; for on this Commission of eleven members there is only one Scottish M.A., who is also a Professor in Oxford, and one Scottish medical professor, who is also M.B of the University of London; the others being a peer, a bishop, an eminent English judge, a distinguished statesman, an equally distinguished naturalist, and others quite removed from the least suspicion of any but a dispassionate judgment upon the facts presented to them in evidence, so far as we or you are concerned.

The two addresses which I propose to reprint along with this one will, I trust, convey to you some idea of the direction and scope of these advances in medical education; not, perhaps, by any distinct and separate statement of them, but by the glimpses they afford of the aspirations, hopes, and fears on your behalf of one who has been associated with medical students, almost without interruption, since he was a student himself; and this, not in the class-room alone, but in the hospital, and at the bedside of the sick, for nearly thirty years. This double relation to you as your teacher, continued over so long a period, may perhaps be considered to give the weight which comes from experience to suggestions in themselves not otherwise remarkable or profound; at all events you will, I have no doubt, receive them as tending to show how much the mind and heart of a University professor are put into his work; and how much the labour of teaching is for all of us lightened by the secure conviction that we possess your sympathy in all honest efforts to raise the work to a higher level. I am also glad to be able to tell you, and especially the juniors among you, that the testimony of all my colleagues goes to show that an increasingly high standard is being attained, gradually, almost from year to year; and that, in particular, the studies of the first year in anatomy, biology, and chemistry, show, to a very marked degree, increasing thoroughness. As I attach the greatest possible importance to these studies, not only in respect of their actual details of fact, but still more as a training of the mind into the paths of observation and reasoned experiment, which lie at the very root of all safe and sound medical education, let me press upon you the importance of continuous diligence and zeal at this, the very commencement of your studies. Be assured that the labour of acquiring even what is commonly called *practical* information and skill in your profession will be greatly lightened, and your success greatly aided, by application to what is, and must ever be, the only foundation of a medical art that reaches beyond the merest empiricism. And now that all these departments may, according to the arrangements of this, and I believe all the other Scottish Universities, be studied in the laboratory as well as in the class-room, you may be assured that all the benefits which, in the first of these addresses, I ventured to anticipate for the development of practical methods of teaching, will be yours. It is, indeed, the increase of these methods in every department of our work that is mainly characteristic of these latter years of our Scottish University medical education. New buildings, new arrangements, new apparatus, new instruments of research are everywhere found necessary, not only to convey the old truths, but to interrogate nature directly as to what is, and what is not, truth. And this *habit of mind*, which is now-a-days not less than essential for success in every medical inquiry, can never be so easily and pleasantly acquired as in the first year of your studentship, in the fascinating pursuit of biological research, whether in the animal or the vegetable kingdom. "Don't think, but try," said that mighty man of biological science, John Hunter; not that he meant to discourage thought or intellectual activity, for his own was of the most unresting character; but that in his opinion all intellectual energy should resolve itself into active work, in the way of experiment and observation. This habit, so different from that of the mere literary student, finding his inspiration and materials alike in the knowledge that is got from books, must be acquired as completely as possible before you come to engage in clinical studies, for it is only to a limited extent that we can, in these, afford you the means of acquiring it. How are we to teach you effectually the fleeting phenomena of disease, if you have not for yourselves investigated the corresponding phenomena of health? How instruct you either in the one or the other to any purpose unless you have systematically cultivated your own faculties of observation and experiment by active personal research?

II.—HAS THE ART OF MEDICINE ADVANCED DURING THE LAST HUNDRED YEARS?[1]

THE importance always attached to the Address in Medicine in this Association has given me many a qualm of conscience as regards undertaking it. But I admit that it is not unreasonable on your part to desire to listen to the opinion, such as it is, of one who has been engaged in teaching the art and science of medicine from youth to a reasonably advanced age, upon the doctrines and practice of the art during the years he has been so at work. This desire I shall endeavour in some measure to satisfy; in some measure, I say, for if you expect from me anything like a *catalogue raisonné* of medicines or of processes, or even of methods of healing, such as they are among us at the present time, you will certainly be disappointed. I shall not even attempt such a catalogue, interesting as in some respects the task might be, for even the attempt would necessarily wear out your patience, while it would leave me open at the end to the charge of having omitted here or slighted there, or unduly enlarged upon in another place, something in which respected friends in this Association might have a special interest, *pro* or *con.* I am not willing to incur such a responsibility, even if I felt able for the task; indeed, any one who has attempted to deal with the subjects included in a course of practice of medicine from year to year, in presence of students of medicine, knows better than most how disastrous it is to attempt to convey into a single address more in the matter of details than the allotted number of minutes will reasonably hold. And I feel, too, that from a teacher, if from any one, you have reason to look for something more interesting, if not more useful, than such a *catalogue raisonné.* He has been in the habit, from year to year, of surveying officially the healing art as a whole. He has watched the rise and fall of doctrines, of methods, of tendencies and currents of opinion. He has come, or ought to have come, to see that there has been an evolution of the art going on in his time; an evolution not at all the work of any one man or any one school, but rather of what Matthew Arnold and the Germans call the *Zeitgeist,* the spirit of the age. Can he at all seize upon those aspects of his subject which most reveal that inner spirit? Can he expound them so as to give a vivid conception of whither we are tending, or how, and by how much, we differ from our fathers? This, at least, seems to me to be the possible theme of an interesting address, though I am very far from believing that I shall be able to do it justice to-day. I only desire to show you what it is in my mind to set before you; what I would willingly sketch out for you as an artist blocks out his picture in a first sketch, for it can be little more. And if I can succeed in any measure even in this, it will have one great advantage for you and me; that upon the basis of the sketch thus presented each of you can fill in details according to his own special knowledge or field of experience. The field, indeed, is vast, as any one must be fully aware who has perused with attention even one thoroughly modern work on the practice of medicine, or, still more, any considerable portions of the vast encyclopædias of Ziemssen, or Dechambre, or Jaccoud, or Eulenburg, or even the less bulky collections, comparatively speaking, in English of Reynolds or of Quain. Are there any ruling principles at all throughout this vast field, or is it a mere aggregate of laborious investigations guided by no common purpose, no fixed or polar star? We recoil instinctively from this conclusion, and yet it must be confessed that the various doctrinal systems of the past, and not a few of the gross charlatanisms of the present, have had a seductiveness for the public, and even for some minds in the profession itself, founded mainly on the notion that here, at last—namely, in the particular formula or perhaps exclusive remedial method advocated—is to be found a ruling principle, a law or compendious statement of doctrine intended to illuminate every corner of the art of medicine; hence it follows as a corollary to minds so preoccupied, that we, *physicians* as we call ourselves in English and in the very old French of the thirteenth century—that is, naturalists or observers of nature—are bound to bring all our accumulated stores of knowledge into court, and make them all do homage to this one doctrine, one law, thus set forth as the only true law of the healing art. I need not now illustrate this remark by any of our old exploded controversies. Every one of you, disclipined in the paths of genuine medical science, knows that to such a universal proposition, when made, there can be only one answer. No such exclusive or single principle or law of the healing art can be said to exist. The action of remedies, the resources of hygiene and of preventive medicine, refuse to submit to any formula comparable in its simplicity with that of the law of gravitation. The healing of disease, in other words, is still largely empirical, and what is more, takes no shame to itself in being and remaining so. It is guided and moulded, every day more and more, by advancing science, but it remains in the end only the application of a more carefully instructed experience. Rational and empirical medicine are not opposed to each other, as one distinguished teacher in my own early days used to represent them. They rather interpenetrate each other more and more every day and every year, and each is the better and the sounder because of this interpenetration.

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[1] Address on Medicine, delivered before the British Medical Association, meeting in Dublin, August 3rd, 1887.

A Curious Inquiry—Hippocrates.

In reflecting over this address, even so far as these introductory sentences, I have been led to think of another "Address in Medicine," delivered before this Association just twelve years ago,[1] by one who was then at the head of his profession in Scotland—my late dear friend, Dr. Warburton Begbie. Some of you who heard that address may have been struck by the almost grotesque character of a question imported into it from the writings of a very great philosopher—a question to which Dr. Begbie attempted a reply at the time; the immediate result both of the question and of the reply being to set me a-thinking, to what effect you shall presently hear.

"Has the *practice* of medicine (that is, the art as distinguished from the science) made a single step since Hippocrates?" Such, alas! was the doubting, almost mocking, query suggested by the late most eminent Sir William Hamilton, Professor of Logic and Metaphysics in Edinburgh University, in an article reviewing Cullen's writings, published in the *Edinburgh Review* in 1832, and afterwards reprinted in a volume of

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[1] *British Medical Journal*, 1875, vol. ii. p. 164.

Discussions published in 1853, not very long before the author retired from public duty with shattered health. To this question, thus propounded in cold blood by a distinguished and marvellously learned authority, Dr. Begbie then addressed himself with a quite wonderful amount of Hippocratic erudition, and certainly with great skill and tact, contrasting modern with ancient practice. I was much instructed by his answer at the time; and yet it would scarcely be correct to say that I was wholly satisfied with it. The interval which he endeavoured to bridge over, from 400 B.C. to 1875 A.D., was quite too enormous to be so dealt with. A fair case was made out on behalf of certain modern inventions and improvements; but the history of progress, on the whole, was not so clearly displayed; perhaps, indeed, could not have been at all adequately displayed, after the manner attempted. Hippocratic medicine, after all, and nineteenth century medicine are not comparable quantities. We still use not a few of the old resources and tools of the art, and have acquired a still greater number of new ones; but we have no details as to the actual use made of these resources in ancient times; no statistics, no accurately-kept hospital records, no scientific basis, so to speak, by which to test the question of the Hippocratic practice in detail, as compared with our own. Nothing is more difficult, perhaps, even in our own day, than to attain to positive conclusions in regard to the behaviour of the same disease under different modes of treatment. How then shall we even attempt to compare, as regards its absolute success, the cure of acute rheumatism by salicin, or of chorea by arsenic, or of syphilis by mercury and iodine, or of aneurysm by iodide of potassium or by Tufnell's method, with anything that we can read of in the works of a Greek, or even an Arabian, physician ? Dr. Begbie was most kindly disposed towards the ancients, and had no difficulty at all in showing that they had worked marvellously well, and had anticipated us of this century in several of the inventions and observations that we are apt to claim as our own. He was also most faithful in expounding many of the claims of the newer medicine to therapeutic success ; and no one certainly of his time was in a more advanced position, or was better fitted by personal experience, to give effect to the claim. But I think the outcome of the whole was scarcely that of an effective answer to Sir William Hamilton's mordant suggestion. It was much more easy to show that we knew more, than that we cured better than Hippocrates and Galen. I never had, and have not now, any doubts on the subject, but if I had gone to work in the spirit of Sir William Hamilton, I think it would not have been difficult, after the manner of the *advocatus diaboli* before the Roman Curia, to have made out something like a case in answer again to Dr. Begbie. And this because, as I have already said, we really know almost nothing as to the degree in which, or even the circumstances in which, the physicians of the Hippocratic era successfully grappled with the diseases of the people at large. We know something, indeed, about the doctrines and methods of one or two of the greatest of them, but we know almost nothing of a secure and well-established kind as to how far the influence of these great men extended, or was applied in the service of sick humanity in general. Within a century or two after Hippocrates, we know that the whole of his compacted doctrine had, as it were, been blown to the four winds in a multitude of discordant sects; and it was not till the great and eclectic system of Galen in the second century of the Christian era gathered up the fruits of this dispersion into a body of doctrine which held its own, almost unquestioned, throughout what we call the Middle Ages, that the art of medicine can be said to have had even any appearance of stability at all. Certainly any one who may have lived at any great seat of medical learning, or of medical practice, in the time of Celsus, or of Cælius Aurelianus, must have had not a few reasons for putting Sir William Hamilton's question to himself. He must have found the opinions and the doctrines of the great Greek master of the age of Pericles very often disparaged, and his most cherished principles and remedies often discarded; cut-and-dry systems, like that of the so-called Methodists, founded on *strictum* or *laxum* as a cause for almost all diseases often occupying the field, together with numerous bold innovations in treatment, not less out of accordance with the rigid or orthodox Hippocratic tradition than those that have become known to us in the present day. It was given to Galen to build up again the fame of Hippocrates; and had it not been for the overwhelming power and authority of Galen's immensely numerous writings, as well as the really pious regard which he always showed for the older and (I think) greater man, and the valuable commentaries he wrote upon many of the Hippocratic treatises, it is very doubtful whether the original father of the Greek medicine would have come down to us as anything much more than *nominis umbra.* For this, even if for nothing else, Galen would deserve the thanks of every student of medical history, and every admirer of one of the most transcendant names of all antiquity—the Æsculapian priest who first emancipated the art of medicine from the trammels of priestly control on the one hand, and of philosophic dogmatism on the other.

CULLEN AND HIS AGE.

Now, gentlemen, it occurred to me at the time of Dr. Begbie's address that a less ambitious programme—one, indeed, not precisely in the terms of Sir William Hamilton's suggestion, but nevertheless including or comprehending what is most important in it for us—might be devised, which would give an opportunity for surveying at leisure some of the landmarks of medical progress, not during the twenty-three centuries since Hippocrates, but during the one century that has elapsed since William Cullen, Professor of Medicine first in Glasgow and then in Edinburgh University, was at the height of his fame. This theme I was so bold as to propound in a few words hastily prepared[1] and spoken the very day after Dr. Begbie's address. It is this theme to which I propose to revert on the present occasion, so far as the time will allow, and as my own slender capacity for dealing with it will permit.

The reasons in 1875 for adopting Cullen as our traditional starting-point lay on the surface. First of all, we were then meeting in Edinburgh, the scene of his longest activity and of his most eminent fame as a practitioner of the healing art. Secondly, it was in reference to Cullen, and in reviewing his works, that Sir William Hamilton was led to raise the philosophic doubt referred to—namely, as to whether the art of medicine had made a single step since Hippocrates. Regarding Cullen, therefore, as the inheritor of the Hippocratic tradition in his own day, it might fairly be argued that, if it should appear that medicine had advanced in any considerable degree since Cullen's time,

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[1] An Address delivered at the Opening of the Section of Medicine, British Medical Association, August, 1875. *British Medical Journal*, 1875, vol. ii. p. 200.

a fortiori the art might be said to have made, not one, but several steps in advance since the remoter era in question. Thirdly, there could be no doubt at all that Cullen was in fact, more than any one in these islands—perhaps more than any European physician since Hoffmann and Boerhaave—the inheritor of the Hippocratic tradition; and that he had connected this with the practical and theoretical activities of his own time with remarkable insight and success. Fourthly, I was myself, as it happened, the occupant of the Chair of Cullen—not, indeed, in Edinburgh, but in Glasgow, where he began his great career as a teacher of the medical art, leaving it for Edinburgh in 1756. This last particular I mention, of course, not as having any public interest, but as having a certain influence in determining the line of my reflections on the occasion of my own very brief address in 1875.

Bear with me, then, while I so far repeat myself as to quote for you only a very few sentences from that brief address; the whole text of which may be found by any curious investigator in the *British Medical Journal* of the date in question.

"It is impossible for any one, being in Edinburgh, and inheriting the traditions of the Edinburgh Medical School, not to associate Cullen with the *genius loci,* of which, indeed, he was eminently a representative man. The fame of Cullen was, moreover, world-wide, and extended beyond medical circles; he was a distinct and noteworthy personage in that literary and scientific society which included Adam Smith, David Hume, Robertson the historian, and Black the distinguished chemist. Cullen had, by reason of his accomplishments, something in common with all of these and all the other eminent men of that time, and was highly esteemed by them all. In point of medical experience, he was at the very head of his profession; a most distinguished clinical teacher, constantly associated with his students in the observation of hospital cases, and, besides this, engaged in private consultations which brought him into extensive correspondence (as his manuscripts in the College of Physicians show) with all parts of the country. Now, one of the most remarkable facts in the history of the epoch of Cullen is one which was not alluded to by Dr. Begbie, but which, nevertheless, might be made available in discussing, or in reflecting over, the signs of progress in the medical art to which he alluded. About a hundred years ago, when Cullen was at the height of his fame, the Chair of the Royal Medical Society was filled by a man who had been the pupil, the friend, almost the dependent, of Cullen, and who then and afterwards became his most bitter antagonist. He was not a man of large personal experience in the art of medicine; his learning and accomplishments were of a very limited kind; his private life and character were the reverse of laudable; his personal and social standing was in every point so much inferior to that of Cullen as to admit of no comparison. Yet this man, first in the Royal Medical Society among the younger students, and afterwards by a literary and doctrinal influence which extended in a wave of controversy to the very extremities of European civilization, became the recognized author of a system which all but subverted that of Cullen for a time in Edinburgh, and, up to the end of the century, occupied the minds of men in Italy and Germany to an extent that we can hardly even comprehend now-a-days from anything we know of the system itself. I will take it upon me to affirm that there are not two men in this room—if, indeed, there are so many in the whole British Medical Association (and I do not pretend to be one of the two)[1]—who could at this moment give offhand a reasonably clear account of the doctrine which was called Brunonianism, and of its application to the leading varieties of disease and their treatment. Yet it would be an inquiry fraught with the deepest interest to us, and I know none which would more effectually gauge for us the progress of the art of medicine during these last hundred years, and thereby show us if we are in reality getting upon solider and surer foundations, than to ask ourselves whether, if some such man as this John Brown were to arise now and to broach such a system as he advocated in 1775, it would have a shorter life, and be less revolutionary in its effects. I cannot enter upon that question to-day; I cannot even talk to you, however briefly, of the respective merits of Cullen's and of Brown's teaching. . . . But if any one should ask—Could such a practice, however indulgent to the weaknesses and follies of self-indulgent people, be successful in our day in floating such a general theory of disease and cure as the Brunonian system? I should incline to reply in the negative; and I should rely in giving that answer not only upon the great advances that have been made in physiology and pathology, the sciences upon which all genuine improvement in the medical art is founded, but also upon the much more exact and complete methods of clinical observation pursued in the present day, even as compared with the time of Cullen. I refer especially, of course, to the methods taught to the average general practitioner in our schools; for no one would wish to deny that we have had great and true, nay, eminently exact, clinical observers in the past, even in the remotest ages."

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[1] Perhaps, however, Dr. B. W. Richardson (had he been present at the delivery of this short and hastily-prepared address in Edinburgh) might have claimed to have been "one of the two" here in demand, as he has since contributed to his own admirable journal, *The Asclepiad*, 1887, p. 357, a memoir of John Brown, which, though perhaps on the whole too appreciative and even indulgent, may be frankly recommended to the perusal of every one interested in the subject.

BRUNONIANISM.

Do not suppose for a moment that I mean to occupy you on the present occasion with such an utterly unprofitable discussion as would be in this our day one on the Brunonian system. For, immense as was the reputation of this very unworthy John Brown[1] in the later decades of the eighteenth century, it may be said to have soon died out, and the man himself to have been almost forgotten in the land of his birth. In France, the new birth of pathological anatomy, represented by the great name of Bichat at the very beginning of the nineteenth century, and afterwards by a long succession of brilliant and successful workers, put a stop to all such vagaries by furnishing a more tangible and real pabulum for the medical understanding. Only in Vienna, and afterwards in the north of Italy (strange to say, in the very country of Morgagni) were idle and resultless disputes continued in the schools, founded on the notion of *plus* and *minus*, as applied to the unknown and utterly incalculable quality of "excitability." But in Italy the controversy, in one shape or other, lingered on, in Bologna and elsewhere; and the division of diseases and of remedies into "hyper-" and "hyposthenic" was still largely responsible for the fame of Rasori, Tommasini, and others, up to the time of men who are, perhaps, still living. It may amuse you to hear that I once, but only once, encountered in the north of Italy a physician in whose eyes Brunonianism was still a very notable heresy, and John Brown almost a towering medical genius, however mistaken. This was in the great hospital at Milan, in the year 1852. I had gone there almost by accident, and was introduced by the late Professor Sigmund, of Vienna, to one of the visiting physicians—a typical example of the "old school." He was, indeed, a phenomenon in my eyes, as I daresay the Vienna professor (not to speak of his younger companion) was in his. What remains in my memory as to his hospital visit on this occasion was that he ordered about every third or fourth patient to be bled; but he did not once use, nor seem even to think of using, the stethoscope. On the contrary, I remember well that when I suggested, in a case of acute or subacute rheumatism, that it might be well to listen to the sounds of the heart, he, with a gesture of impatience, repudiated the suggestion that there could be anything wrong there: "*Tutto é possibile*" was his expression; "*ma ciò non é*"; and in the same breath he gave orders for "*salasso alle dodici*" (bleeding to twelve ounces). When I was first introduced to this old gentleman in the character of a young graduate of the Edinburgh school—then nobly represented all over Europe by Christison, Alison, Syme, Simpson, Goodsir, Bennett, all at the height of their fame in various lines of medical enterprise—it appeared to me just possible that their names might be known within the precincts of the Ospedale Maggiore of Milan. But, so far as this venerable representative of that hospital staff was concerned, I had pretty good evidence that it was not so. "*Edinburgo,*" he said to me, in seeking for a compliment to my *alma mater;* "*Ah, si, sicuro—gran scuola, Edinburgo*; *Cullen era d'Edinburgo*; *si—Cullen*—e Brown! *veramente, gran scuola!*"

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[1] It need scarcely be pointed out to the present generation, but might perchance require explanation at some future day, that the John Brown of the Brunonian system was *toto cœlo* different from the exquisite humorist and most charming and lovable companion who inherited the same name, though by no means similar ambitions, in Edinburgh—the author of *Rab and his Friends.*

Those were days before any of the revolutions representing modern medical science had made their mark in Italy; before Cavour had perished under the lancet, in such a way as to make all Europe cry out against the abuse of that potent, and also at times very deadly, weapon. Italy and Spain were, in fact, several decades at least behind the age, and the belated march of medical progress had not been quickened there appreciably by the breath of public opinion in France, Germany, or England at this date. Nor was my venerable friend in the Ospedale Maggiore the only representative of the old school then flourishing in that great institution; for, as Sigmund himself told me, on meeting me after a brief visit to the venereal wards, it was still possible to see there a man who had just been ordered to be bled for the third time in a gonorrhœa! Evidently in Milan, and in 1852, therefore, it might reasonably have been argued that the art of medicine, in its main traditions and practices, had not made many steps in advance since the time of Cullen, or possibly even of Hippocrates.

THE HIPPOCRATIC TRADITION: BLOOD-LETTING.

I should not have lingered over this personal incident had it not been that it enables me to give you a slight foretaste of an argument to which I wish to return in the course of the present address. There are very few of those now before me, I suppose—certainly not one of the younger members of this Association—who can possibly remember, I do not say any incident at all parallel to this, but anything at all approaching it in the shape of blind faithfulness to what was at one time supposed to be the Hippocratic tradition. Mark me, I do not say that the practice of medicine by Hippocrates himself was truly thus represented; my own clear opinion, entertained for many years, and without, I think, any undue prejudice, is that it was not so. It is rather remarkable that in the genuine Hippocratic writings we have only one example, if I am not mistaken, of blood-letting freely used under circumstances fully stated to us in detail—the case of Anaxion, in the third book of the *Epidemics.* The fact has been often noticed, and in the palmy days of venesectionist practice was explained away by a process of *ex post facto* reasoning, not very dissimilar in kind from that by which the facts of modern geological science are brought into an apparent harmony, or, at least out of apparent collision with the book of Genesis. I have no time, however, to fairly discuss this point, or any other point of merely scholarly importance in the present address. I must ask you to take it as the matured conviction of one who has given some attention to the subject, that the genuine Hippocratic tradition was not nearly so extreme[1] in respect of blood-letting as that of the mediæval, and still more that of many of the comparatively modern, inheritors of the doctrines ascribed to him. It has already been stated that in the four or five centuries which preceded the firm establishment of the Roman empire, the practice of medicine had abundant time and opportunity to grow out of the leading strings of the supposed Hippocratic authority. Sects were rampant, and in the city of the Cæsars they were peculiarly so. Every doctrine of what was regarded even then as a venerable antiquity was thoroughly, and even keenly, discussed, with a freedom—though not, in all probability, with a freedom from prejudice—similar to what we may observe in the highest class of medical minds in our own day. Blood-letting had to hold its own against the most uncompromising opponents, and in some instances, no doubt, opponents on principle, of whom it is only necessary to mention by name Erasistratus, and this chiefly because, as an anatomist of celebrity, even in these early ages, he may probably have represented something like the attempt to raise up, independently of the Hippocratic tradition, a school of what we should now call scientific medicine. But in answering the question propounded by Sir William Hamilton, and again by Dr. Begbie, it would be absolutely necessary to take count not only of what Hippocrates did or taught personally (a thing not always easy to find out), but also of what he established and transmitted as a legacy to his remote successors. Now, as I have already said, it was certainly not to Hippocrates but much more to Galen, that any apparent stability of the art during the succeeding centuries is due. For good or for evil it was Galenist doctrine, much more than Hippocratic doctrine that moulded the ideas of the later Greeks, of the Arabians, and of the whole Middle Ages and revival of letters. And it was from

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[1] Take the following brief statement of it, which, although not certainly by Hippocrates himself, is probably quite in the spirit of his teaching: "You will bleed in acute affections if the disease appears to be severe, if the patients are in the vigour of their age, and if their strength is well maintained." (*Regimen in Acute Diseases*, Appendix, sect. 2.) Anaxion, above referred to, was bled on the eighth day of what appears to have been a very acute pleuro-pneumonia. There was only one bleeding, so far as appears, though it was characterized as being a "large one, as was necessary." There was a return of fever on the twenty-seventh day, the complete crisis occurring on the thirty-fourth, after an abundant expectoration, "with the characters of coction." The above may be compared, or contrasted, with the practice of James Gregory, to be referred to further on.

Galen, far more than even from Hippocrates, that Cullen received, indirectly as well as directly, the body of traditional doctrine on which he worked by way partly of criticism ; and the practice of blood-letting, which he, like most of the men of his time, largely adopted.

INSTABILITY OF THE MEDICAL ART IN CULLEN'S TIME.

Now, to return for a moment to the Brunonian system, or heresy, and the apparent defeat which, for a time, it appeared to inflict upon the traditional or more orthodox medicine, represented by Cullen. The moral that I wish to point, as derived from this incident, is that the art of medicine, towards the close of the eighteenth century, was in a condition of very unstable equilibrium. Cullen, no doubt, did his very best for it, and compacted it into a system so luminous and so admirably set forth in his *Nosologia Methodica* and his *First Lines*, that, if I mistake not, it will be found that the influence of these two books has saturated, so to speak, almost the whole of the British, and certainly not less the Irish, tradition down to the time, at least, of Sir Thomas Watson in London, Alison and Christison in Edinburgh, Graves and Stokes in Dublin. The eighteenth century, in fact, was a century, in relation to the medical art, of system-building. Stahl, Hoffmann, and Boerhaave built each upon the foundation, or rather in great part upon the ruins, of his predecessors ; and yet of all these great compacted systems of medical doctrine[1] only that of Cullen, and perhaps of Boerhaave (through the extremely copious and readable commentary of Van Swieten), retained any authority or practical importance at the end of the century. It seemed as if the medical mind, oscillating between the claims of humoralism and solidism and vitalism, could find no resting-place, but was constantly exposed to be drawn out of the path of traditional medicine, by any new theory which presented a certain amount of apparent simplicity and plausibility, all the more if it contemptuously dismissed, as unworthy of serious study, the laborious and positive acquisitions of clinical and pathological fact which had even then been largely gathered in, especially by Morgagni, as to the natural history of disease; and this was exactly what the Brunonian system did. It represented the treatment, not of one disease, but of almost all diseases as bound up with, and at the same time confined by, one formula or method of which the practical outcome was the copious administration of alcoholic stimulants and of opium.[2] Substitute infinitesimals for the very palpable and potent remedies of John Brown, and exactly the same remark may be made of the homœopathic doctrine, also a revolutionary child of the eighteenth century. But on this subject I have no time to enlarge. I allude to it only as one other illustration of the instability of medical doctrine in the time of Cullen, and this in Germany as well as in England.[1]

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[1] A carefully written account of them will be found in Dr. John Thomson's *Life of Cullen*, vol. i.

[2] That there was not only *method* (as stated above), but also a good deal of cleverness, and even of easy-going indulgence for human infirmities in Brunonianism as practised by its author, may be inferred from a prescription of his which is still extant, as follows :—" For breakfast, toast and rich soup made on a slow fire ; a walk before breakfast, and a good deal after it ; a glass

Have we attained to greater stability now? This question I ventured to answer, in 1875, in the affirmative, but with a very brief, necessarily brief, allusion to the circumstances which appeared to lead to this conclusion. I adhere, however, to the following statement, as then made; and I wish now to make it the subject of a few more remarks in this place. "There seems

of wine in the forenoon from time to time; good broth or soup at dinner, with meat of any kind the patient likes, but always the most nourishing; several glasses of port or punch to be taken after dinner, till some enlivening effect is produced by them, and a dram after everything heavy. One hour and a half after dinner, another walk; between tea-time and supper a game with cheerful companions at cards or any other play never too prolonged; a little light reading; jocose humorous company, avoiding that of popular Presbyterian ministers and their admirers, and all hypocrites and thieves of every description. Lastly, the company of amiable and delightful women and an enlivening glass."

[1] The very notable disturbance of tradition by the system of Broussais, in France, at a later period, although running entirely in the opposite direction, may perhaps be alluded to as a somewhat similar phenomenon, although here the corrective was speedily applied in the admirable researches of Laennec and Louis, based upon imperishable foundations of fact.

reason, therefore," I said, "to believe that we have been gradually gaining for the art of medicine a more secure position in connection with demonstrable or demonstrated facts, and that we have gone some way towards making it impossible for any man, coming forward with a new theory not based upon the careful study of facts, to make such a revolution in medical practice as was the case in Brown's time, and such as has been over and over again witnessed in the history of medicine almost from the days of the Hippocrates."

DIRECTION OF MODERN PROGRESS.

The implication which is carried by the sentence just quoted is that the progress of modern medicine since Cullen's time has been in the direction of *stability*—a kind and degree of stability not possessed by the art of medicine in any previous stage of its progress, not even in those "dark ages" (as we call them) when it had the *prestige* of a kind of immutability, or, as a theologian would call it, *orthodoxy*, founded on the overwhelming authority of Galen. Can I at all make good this proposition to you in the minutes that remain to me for this address? In order to do so at all, I need not say that I can deal only with a very few salient points in a vast field of inquiry. I must leave on one side to a great degree the enormous outgrowth of new remedies, new processes, new theories and practices in respect of disease which may at present be occupying all of your minds, as the proofs of our advance, but which, on the other hand, an adversary might declare to be still on their trial, and possibly even a questionable indication of progress, inasmuch as a profusion of means is not the same as a positive attainment of results. To take a single illustration from surgery, which is out of the direct line of my inquiry, but which cannot fail to be a striking one to most of you: it is not open to me even now to claim for *antisepticism,* in the strict sense of the word, the credit of an advance all along the line, because, as you all know, we have eminent surgeons among us who claim to obtain, and I believe do obtain, brilliant results without using the particular remedies called antiseptics; and therefore the theory of antisepticism, and the separation of what is permanently from what is only incidentally true in this great issue is a research of no small complexity, and perhaps of extreme difficulty for a professor of medicine, who nevertheless believes, as I do, that the impulse communicated to surgery through antisepticism is one of the greatest in the history of the art. And yet it is surgery which, as respects the more tangible and obvious character of its processes and results, has been humorously defined as "*Quod in medicinâ non dubium!*" and I am bound to speak to you, if I speak at all, of facts and theories even less doubtful than this.

As regards that great revolution in the medical treatment of fevers and inflammations, which in some minds occupies in relation to medicine as advanced a position as antisepticism does in relation to surgery, I can only declare in this place that, while I have no doubt at all as to the advantage in some cases, and, in a limited sense, of the so-called antipyretic or antithermic treatment, I have yet to be convinced of the advantage of its application as a routine in typhoid or in any other fever. In other words, it has not as yet been made quite clear to my mind by evidence, statistical or other, that the treatment of any fever or inflammation by such means as were recorded some years ago by Liebermeister and Jürgensen, the one as regards typhoid fever, the other as regards pneumonia, has resulted in a permanently lessened mortality when set against results obtained or obtainable in this country, without these very active procedures. Having stated my opinion to this effect some time ago, however, I feel bound to add now that I am by no means insensible to the wonderful patience, research, and apparently scientific exactness shown by these authors in their latest researches on the subject. Certain modifications of the practice have also tended towards the removal of distrust, and should another great epidemic, say, of typhus fever, occur in Scotland, I should very gladly witness or concur in a therapeutic experiment such as would practically settle the question for us. In the meantime, however, and on grounds which I had occasion to state at the Belfast meeting of this Association,[1] I must regard the antipyretic treatment in all its various forms as still under controversy. For the purposes of the present address, therefore, it is still, even if full of promise, not capable of being claimed as a clear step in advance—"*adhuc sub judice lis est.*" But it may be readily conceded that both in typhoid fever and in pneumonia, as previously treated in German hospitals with an enormously high mortality, the method in question marks a distinct advance.

When we come to individual remedies and processes of cure, in respect of internal medicine, we have, of course, to deal with an exaggerated form of the same difficulty; unless, indeed, I am to make mere assertions go some way to supersede arguments. We have no time, in fact, to discuss in this address the action, or to appreciate the exact position in reference to the past or the future, of individual remedies. What shall we say about cod-oil, for example, in the treatment of phthisis? I am very sure, speaking as a practical man, that the general management of all manner of consumptive disorders has immensely improved since the introduction of cod-oil into their medical treatment. But is the oil a specific, or is it merely a food? and how does the oleum morrhuæ take rank—whether food or medicine, or both, as a therapeutic agent among the almost innumerable new means, and, as I think, distinct advances, in the management of consumptive diseases within my own personal recollection? I am well assured that most of you will agree with me that these questions cannot be adequately answered now. At all events, here is one of the most popular of remedies, which has been fully before the British medical public almost as long as the British Medical Association itself; and yet I am not in a position, even now, to dogmatize about it as marking, *per sc*, a distinct step in advance in subduing one of the commonest of diseases, without a host of rival agencies immediately claiming attention, as sharing with cod-oil the glory, if any, of the supposed advance.

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[1] *British Medical Journal*, 1884, vol. ii., p. 1125.

No one having any familiarity with modern medical practice can overlook the large place occupied in it by certain powerful, and it would be hardly too much to say indispensable, remedies acting on the nervous system, many of which are entirely modern; that is, new not only to therapeutics, but comparatively new in respect of being unknown, even as substances, until a date very much within the bounds we have assigned to the present inquiry. The most prominent, and perhaps also the most unquestionable of these is chloroform; and if forty years can ever be regarded as sufficient for testing an advance in medicine, or surgery, it might be safely affirmed that the reputation of our day and generation might be regarded as saved by this one great achievement. Yet there are not wanting evils to be dis-counted, and failures to be set against the benefits of this enormous boon to the suffering human race; and it would not surprise me to find that there exist, even now, well-informed persons who believe that the Lethe which comes from modern chemicals, like that which of old was born of poppy and mandragora, has been a questionable advantage to humanity at large. What then shall we say of hydrate of chloral, butyl-chloral, paraldehyde, urethan; and what of the condensed forms or alkaloids of the older narcotics, so immensely convenient, but also so powerful for mischief? I must leave you, gentlemen, to answer these questions. I, for my part, have no doubt that the balance inclines, at least in instructed medical hands, greatly in favour of the service these remedies are doing, and may yet do, to great numbers of the sick, and especially the fatally afflicted members of our race. And it is also not to be forgotten that in the treatment of one of the most incurable diseases, as well as in many others, we have secured, since 1857, a palliative (to say the very least) of the greatest power in the bromides. The almost accidental discovery by Sir Charles Locock of a very special therapeutical application of bromide of potassium has in this case opened up quite a new chapter in the resources of the medical art as regards a host of other diseases, and chiefly of epilepsy, which from the time certainly of Hippocrates had always been regarded as one of the chief reproaches of the art, and accordingly given over to every kind of physical and metaphysical superstition.

Again, the modern use of digitalis, strophanthus, and other cardiac tonics; and again, the use of nitrite of amyl and of nitroglycerine in angina pectoris and the allied conditions: Who is there among you that doubts (for I certainly do not) that in these, when well employed in judicious and well-instructed hands, we have obtained a very distinct and very valuable addition to our resources in cardiac diseases? And yet, is there any one of you who will expect me to prove this in an unassailable manner, to the satisfaction of a sceptical friend or foe within the limits of the present address?

Once more: the benefit derived from the salicyl compounds in acute rheumatism is, to my mind, very satisfactorily established; and I should be personally disposed to claim this as a very distinct advance in the healing art within a comparatively recent period, and a partial removal, moreover, of one of the old *opprobria medicinæ* which I have lived long enough to have been obliged, over and over again, to recognize as such. But will any one say that my simple assertion of this (for we could not hope to get further into the argument now) can reasonably outweigh all the sceptical or contradictory statements on this point which it might be expected to call forth? Evidently, in regard to remedial agents and processes not more than twenty years old, it is vain to expect such a concurrence of opinion as to entitle any one to set them forth to you dogmatically, as the permanent and unquestionable milestones in our progress, which future ages too may be expected to acknowledge as such. In considering stability, therefore, we have not only to lay our account with possible new discoveries, but also with the perhaps unknown or unappreciated dangers attaching to practices now vaunting themselves in the full sunshine of apparent prosperity and success. While, therefore, I have and profess the most entire faith that the efforts of hundreds of young and enthusiastic workers are being fruitfully directed towards a science of pharmacology, on the joint bases of experimental research and scientifically directed observation, we are, still I fear, too much in the transitional stage to allow of many of these researches being adduced in evidence of the increasing stability of the medical art. Rather might it appear to some, even of ourselves, as if the multitude of new remedies which come to us from all the ends of the earth, the too brief reputation of some of these, and the evidently over-sanguine estimates of others, are the proofs of an unrest which is quite the reverse of stable; which cannot be satisfied with the old remedies, old prescriptions, old methods of cure; but holding itself bound to drive on the *currus triumphalis* of modern therapeutics at all costs, is likely to land us once more in a new series of controversies, such as attended the famous work of Basil Valentine on the healing virtues of antimony, perhaps three hundred years ago.[1]

But I hold, and have never ceased to hold, as a teacher of medicine, that all this fermentation, so to speak, of opinion with respect to current remedies and modes of treatment, all these tokens of unrest and dissatisfaction are, in reality, an advantage to the medical art, and tend both towards progress and towards stability; an altogether different kind and measure of stability, however, from that implied in the authority of Galen, or the traditions of the Greeks and Arabians. Allow me again to quote a very few sentences spoken rather more than twenty years ago, but to the general sense and scope of which I am disposed still to adhere, with the proviso that the twenty years in question, and especially the newer labours in pharmacology, which I only do not name in detail in order that I may not be supposed to be indulging personal preferences in a semi-popular exposition, have done much to carry my convictions as to a real and solid advance, as compared with the earlier date.

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[1] I have given a brief description of this strange controversy in an Address partly reprinted in the present volume. The date, and even the personality of Basil Valentine, are extremely obscure; but it is certain that the administration of antimony was formally condemned by the Faculty of Paris in 1566, and again in 1615, both Acts being ratified by the Parliament of Paris, and numerous prosecutions, with endless paper-warfare, following thereon. In 1666 the use and administration of antimony was formally allowed, the disputes having extended over more than a century.

ORTHODOXIES v. REAL SCIENCE.

"The art of medicine," I said, "is at this moment in a peculiar position. The day of orthodoxies is over, the day of real science is only just dawning. It is no longer possible to condemn a man, even by implication, for having ceased to believe what our fathers believed; but it is extremely difficult to state in general terms what we believe ourselves, and still more difficult to forecast the future, and to lay the foundation of the faith of our successors."[1]

Notwithstanding the apparently sceptical ring of these sentences, I hope to be able to show you immediately that there are at least some points of our progress in which the dissolution or weakening of what I call orthodoxies has been accompanied by a truly constructive process, and that the new theory and corresponding practices which have replaced the old differ from it in this—that, being the result of applied science and not of mere tradition, they are correspondingly secured against revolutionary overthrow.

1. *Hygienic Remedies.*—The fruitful, and I think unquestioned, progress of preventive medicine (other-

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[1] *Medical Education, Character, and Conduct;* Introductory Addresses, delivered to Students of Medicine in Edinburgh and Glasgow, 1855, 1866, 1882, p. 58. Glasgow, 1883.

wise called sanitary science) during the last thirty or forty years is not only a very striking advance in itself; but it may also be maintained, I think, without contradiction, that curative medicine (with which we are chiefly concerned here) has largely and quite steadily advanced along the same lines.

The prevention and the cure of disease are not, indeed, identical, but the laws which guide the former are in a large measure, and necessarily, applicable to the latter. The same noxious influences which beget typhoid fever, for example, tend in a remarkable degree to exaggerate, if not to cause, a host of minor diseases, and very notably to increase the risks of the healing of wounds, and of the puerperal process. The same overcrowding, which is in a great degree the cause of typhus fever, is a source in hospital wards of pyæmia and erysipelas, in barracks and gaols of consumption (and this often among picked men), in certain endemic localities of contagious pneumonia, and possibly of diphtheria.

It is impossible, in the face of modern sanitation, to escape the conclusion that the causes and the remedies of disease alike are often to be sought more in the study of groups and even of multitudes of cases than in the ordinary text-books, or in the conventional remedies set down for the particular *nomen morbi.* In short, the treatment of diseases of every kind has been largely, even if silently, influenced by the thought that the chief disease-breeding factors must also be, in individual cases, adverse influences to be carefully studied and kept in check by those who would cure disease; and that, in chronic cases especially, a large part of the cure consists in the removal or modification of the injurious action of the cause. I had an opportunity, however, of enlarging a little more on this great theme than I can do to-day in my address in 1883 before the Sanitary Institute[1] at their Congress in Glasgow, when I occupied the chair of the Section of Sanitary Science and Preventive Medicine. I will, therefore, abstain from saying more about it now than that the changed point of view implied in the more fruitful and practical study of causes has in many cases made it impossible to cling to the old and traditional aspect of certain diseases as entities,[2] arising we don't know (and don't strive to know) how, and only to be met by a *farrago remediorum*[3] addressed to individual symptoms, or, at least, directed to the consideration that a morbid influence, or something unknown, has got to be battered down in the recesses of the economy by sheer expenditure of therapeutic violence, at whatever cost to the physiological functions of the sufferer.

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[1] *Transactions of the Sanitary Institute of Great Britain*, vol. v., p. 71, 1884; reprinted in the present volume, *infra.*

[2] This subject was discussed more at large in a Presidential Address delivered by me at the annual meeting of the Medico-Psychological Association, August 2nd, 1882, and published in the *Journal of Mental Science* for October, 1882; re-published in the present volume. See also *Insanity; Modern Views as to its Nature and Treatment.* Glasgow: MacLehose. 1885.

[3] The pharmacopœia of Sydenham (who was in his day considered the most enlightened of physicians) contained, not infrequently, thirty or forty drugs combined in one prescription. I have had occasion to remark unfavourably (though, of course, without giving names) upon prescriptions even of contemporaries containing eighteen separate ingredients, several of these

2. *Self-healing Processes in Acute Diseases: Abuses of Venesection.*—Closely allied with this change, which I take to be a permanent one, making in the direction of stability for the medical art, is another which has come to a head, or at least has begun to bear fruit largely, only within my own recollection, and which I can distinctly recall as most powerfully influencing my own mind and practice from the very earliest period of anything like independent thinking about it. I do not wish, even for a moment, to lose sight of the fact that the irrational orthodoxies (as I ventured to call them) of the past have been unsettled, very largely owing to the prevalence and the countervailing influence of certain heresies, of which no rational mind can approve. But it is one of the privileges of our art in its more advanced and, as I think, more stable condition in this day of ours, that where there is no orthodoxy there can be no heresy (in the old ecclesiastical sense of the term); or, rather, that our freedom

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being strong and poisonous remedies. The late Sir Robert Christison's eminently wise, though uncontroversial, attitude towards the polypharmacy of his own day, has been the subject of some remarks in the second volume of his life, recently published, p. 131. Edinburgh, 1886.

of opinion is such that we can afford to absorb into our daily practice, and even into our theories of disease, the instruction that comes from even the most erratic and monstrous of heresies, no man making us afraid. Forty years ago, for example, it was still a point fairly open to question[1] whether pneumonia and other acute inflammations could possibly be cured without blood-letting. You all know, as well as I do, what has come of this burning controversy, and how completely the old traditional theory, as it came down from Cullen and Gregory, has been overthrown; but it is surely a cardinal instance of the essential instability of that theory, that after being for a time supplanted, in Cullen's own day, by the ridiculous extravagances of a vain-glorious charlatan like John Brown, it should have been reinstated in greater power than ever as regards this particular point in therapeutics, only to be subverted once more, and more completely, by a far better man, the late Sir John Forbes. He it was first,

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[1] See the numerous articles, letters, etc., directed particularly to this question, and to the simpler or more expectant treatment of diseases generally in the later volumes of the *British and Foreign Medical Review*, edited by John Forbes, M.D., F.R.S., F.G.S., and especially the very candid statements of the editor himself in the postscript appended to the concluding vol. (xxiv., 1847, p. 594), as to the influence of these discussions on the sale of his journal, and probably on its discontinuance. See also the controversy, famous in its day, on the particular subject of pneumonia, introduced by Dr. Bennett and Dr. Alison into the Edinburgh Medico-Chirurgical Society in 1856-57, and chronicled in the *Edinburgh Medical Journal* of those years.

I think, who, becoming distrustful of the old tradition, and anxious to learn in a genuine spirit of scientific humility about "Nature and art in the cure of disease," did not refuse to accept truth, even at the hands of homœopathists, and of others not much, if at all, superior to the Brunonianists of Cullen's day in their claims upon genuine medical science.

Although it will be impossible for me to enter into the details of this controversy, I happen to have the means of giving you a very instructive and vivid glimpse into the state of opinion out of which the controversy arose, for I have the great privilege of being able to read to you, from the manuscript notes of the late Sir Robert Christison, placed in my hands by his sons, the actual teaching of the Edinburgh Chair of Medicine on the subject of blood-letting in pneumonia in or about the year 1819.[1] Dr. Christison has paid an ample tribute, in his Autobiography, to the excellence and power as a teacher, as well as to the great practical experience as a consulting physician, of Dr. James Gregory; and I will only add, before reading these sentences from his lectures, that the state of mind they set forth as regards the imperative necessity of blood-letting in certain circumstances is one simply impossible now; in saying which, however, I wish to make the reservation that there still exist good men and true who believe that a much more qualified use of the remedy is in accordance both with experience and with the dictates of true science. My own opinion is not, perhaps, of much consequence; but I may state that, having at a very early stage of my career become convinced of the great abuses of venesection, I have not had anything to cancel or recall in this respect; but am rather inclined, if anything, to admit that the reaction of opinion against this and other powerful therapeutic agents may have gone a little too far, as will perhaps occur to some of you also in reading an article by an able and skilful practitioner, a very good friend of mine, in the number just published of the *International Journal of the Medical Sciences.* I forbear, in accordance with the whole plan of this address, from indicating living writers; but I feel sure that this gentleman, and several other friends whom I could name, engaged more or less in country practice, are quite capable of reading lessons to us of the towns, and of the hospitals and schools, which will one day bear fruit. But as to what may be called the Gregorian practice, probably differing (for the worse) even from Cullen's, and certainly from the genuine Hippocratic tradition, I expect that there can be now only one opinion among my hearers.

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[1] The copy of notes in question is not dated, and may have been written in 1818. Sir Thos. Watson attended Dr. Gregory's lectures in 1820, and carried away an equally vivid impression of them with Dr. Christison, as his own famous work shows at several points. For Sir Robert Christison's personal impressions, see his Autobiography, in the *Life*, edited by his sons, vol. i., pp. 76-85; also vol. ii., pp. 117, 118. Gregory died in 1821.

Here then are, as nearly as we can obtain them, the very words and phrases of James Gregory, as addressed, under all the sense of high responsibility as a teacher, to the students of Edinburgh University:—

"By far the most effectual way of checking inflammation of the lungs is blood-letting. In most cases, more than one bleeding is required, though some very mild cases may be successfully treated by one bleeding, and that not a very large one. I have seen some patients cured by a single blood-letting, to the extent of 10 or 12 ounces only; but it is very seldom that practitioners are called in early enough to effect a cure so easily. . . . *Six, eight, or ten times in the course of two or three weeks is commonly sufficient to stop an ordinary pneumonia. It is often found necessary to bleed twice, sometimes three or four times, in one day.* But this very rough practice is by no means to be employed without a very sufficient reason. . . . We must always be aware of the necessity of not urging it too far" (!).

"It is not easy to regulate the quantity necessary to be drawn at each bleeding. From 12 to 20 ounces Cullen reckons an ordinary bleeding—under 12 a small, above 20 a large bleeding; but in many cases it is found both practicable and expedient to go far beyond 20 ounces. The greatest quantity I have ever ordered to be drawn from a patient under pneumonia is 35 ounces.[1] From a man of middle age, and under the middle size, I once took that quantity at the eighth bleeding, and on the seventh day of the disease. Before the last bleeding the symptoms were very severe, and they were accompanied by violent hiccough. If this arose from any affection of the stomach, or from general weakness, I knew that the man's life was irrecoverably lost. But there was a chance that it depended on the inflammation having spread to the diaphragm. Considering this supposition as the only chance of cure, I ordered him to be bled till the hiccough went off. It did go off, and the man ultimately recovered. In seven days he lost 160 ounces of blood."

Other instances are given in detail which I prefer to summarize, in order to save your time, although they are well worth reading. In one, 123 ounces of blood were drawn in little more than twenty-four hours. In another, a medical student, 264 ounces were lost in the space of about a fortnight, and at one of these numerous bleedings (for a cerebral affection arising from injury) 60 ounces were taken at once;

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[1] On this the reporter (Christison) remarks in pencil (perhaps at a later date) as the result of his own experience: "I once took 50 ounces from a negro, and 40 more in thirteen hours. Dr. Graham, Professor of Botany and Clinical Medicine, once took 72 ounces for pleurisy."

"a far greater quantity," adds Gregory, "than I have ever attempted to take." In another case, communicated to him by a pupil, 560 ounces of blood were lost, on account of pneumonia, between July 25th and August 16th, 204 of these being in the first week of treatment. But the following example is, perhaps, more startling in its brief and simple record than any merely numerical statements:—

"A man of small stature laboured under pneumonia, and had been repeatedly bled. On the evening of the second day, my clinical clerk came to inform me that, pursuant to my request, at noon he had got him bled to 32 ounces, but that the symptoms were not abated; that he was very weak, and did not appear capable of standing further blood-letting. I went with the clerk to see him, and, notwithstanding the unpromising condition of the patient, I resolved to bleed him again, *ad animi deliquium.* After 22 ounces were taken away, there was no appearance of his fainting; but severe hiccough came on, and occasional twitchings of his face. I was exceedingly alarmed at this; but they disappeared on laying him in the horizontal posture: the pain and hardness of the pulse abated, and he became soon convalescent. In the course of two days and a half he lost 98 ounces."

There is a good deal more in these instructive notes that would bear citation with respect to my present purpose; but I daresay you will permit me to close the interesting record with one more very brief paragraph, which is chiefly valuable as showing the well-established reputation of the speaker, and his entire good faith in pressing these conclusions home to his hearers.

"Many of you may probably have heard much said about my rough practice in pneumonia, and the eagerness with which I urge copious detraction of blood. This may be, so far, true. But you will observe that I am seldom called in, except in the very worst cases, *when every practitioner would be aware that nothing but free venesection can be of utility.* If I get any patient in favourable circumstances, I by no means carry it to such an extent. There are many who go along with me in this practice, and there are several very eminent practitioners who carry it to a greater length than I ever do."[1]

Setting aside differences of individual opinion, I have no doubt that you will all agree with me that the most striking fact about these extracts, considered as an historical specimen of authoritative teaching, is not even the unhesitating faith in blood-letting as *a* remedy, but the belief in it as *the* remedy—the only possible remedy—for a severe case of pneumonia. It is as clear as anything can well be, and I may add that the passages I have suppressed for the sake of brevity only bring this out more clearly, that Gregory believed with all his heart, with a strength of conviction apparently derived from experience as well as tradition, that a case of this kind, if not bled, and bled largely, must of necessity prove fatal. This conviction, and no other, will account for his fanaticism, as we must now esteem it—the fanaticism, however, of a powerful and vigorous mind, misled only by the fact that, under the influence of a traditional orthodoxy, he had never allowed himself to think or to try otherwise. For now-a-days we do not *think*, but we *know*, most absolutely and positively, that it is quite otherwise; that the great majority even of severe cases of pneumonia will get well under a much simpler and less spoliative treatment. We also know full well that under such treatment the mortality is much less now than it was then or since, under a free and a systematic treatment by blood-letting. But the great, because the permanent, step in advance has been in reaching the conviction, founded on experience, that a simple, almost a hygienic, treatment of the most acute diseases may also be one of a very moderate or low mortality. And this conviction, otherwise expressed as one of the self-healing tendencies of acute disease—although probably embodied in a phrase attributed to the father of Greek medicine—*νούσων φύσιες ἰητροί*[1]—has required more than two scores of centuries, and especially the last half-century, to stamp it down as a current medical doctrine; to convert it, moreover, into a fundamental rule or principle of medical practice in the present day.

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[1] I will venture here to express again what has been elsewhere noted with respect to these valuable manuscripts, interesting to every historical student of the art of medicine, "a wish that some selections from them could be published even now, as a monument of one who has left no printed record of himself in the department of practical medicine, in which he was so long an acknowledged leader."—*Christison's Life*, vol. ii., p. 118.

3. *Epidemic Fevers: Alcoholic Stimulation.*—I am tempted at this point to allude to another instance of changed practice occurring within our own day, and applied, or applicable, over a very wide area of disease. I do not know if in this instance I can count upon the assent of all of you to the revolution being a *fait accompli* even now, nor will I venture to affirm that the terms in which it must be stated are final; but as the Public Orator of the University of Dublin furnished some of us only the other day with an epigrammatic statement of it in the words *lacte non vino,*[2] perhaps a very few minutes may be devoted to the consideration of it, as briefly and as impersonally as is possible in this place.

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[1] See *Insanity: Modern Views as to its Nature and Treatment,* p. 29.

[2] The following sentences from Dr. Webb's oration at the honorary graduations on June 30th, 1887, though subject to correction in at least one point of detail, may be permitted here as a humorous statement of the case, to be preserved in connection with the present address: "Febris fanum, ut memoriæ tradidit Cicero, in Palatino quondam stetit—inter colles Glasguenses nunc simile stat fanum. Hospes noster, quem nunc salutamus, ejus est Antistes (?) et quod mirum, lacte non vino numen illud detestatum se placavisse gloriatur."

All of you who have read (as who has not?) the late Dr. Murchison's classical volume on *Fevers* must have been struck with the historical summary he gives, with such minute accuracy, of the confusion arising out of the names of the continued fevers adopted by Cullen—synocha, synochus, and typhus; the first, corresponding with the fever of high reaction, or so-called inflammatory fever (perhaps the *καῦσος* of the Greeks); the last, with the low nervous fever, or fever of debility, and partly with the malignant or putrid fevers of some of the older authorities. Now, as a general rule, it is hardly too much to say that while the employment of blood-letting and other evacuant methods was at one time universal, or nearly so, in the synocha, recognized as such; the treatment of typhus, when frankly admitted as typical, and as tending to debility and nervous exhaustion, has leaned as a rule, during at least the last seventy years, in the direction of alcoholic stimulation. Synochus, as a merely intermediate or indeterminate form, may be here excluded from consideration; but it is important to remark that, up to the time to which my own recollections of current medical doctrine extend, the idea was prevalent that epidemic continued fevers oscillated between synocha and typhus, in cycles of years; and that the treatment, in order to keep pace with the constitution then existing, had to be regulated according to the "epidemic type" which, judged by the experience of those who had been watching the progress of events, could be said to represent the general tendency, according to the all but universally received doctrine of the "change of type." This doctrine, which may be traced back, though not under the same name, to Sydenham, was strongly held in Edinburgh when I was a student; and when the relapsing fever of 1843 (a true synocha according to the old definitions) somewhat suddenly appeared, it was held to be a "nova febris," quite distinct from the mixture of typhus and synochus which had held the field for many years before. Dr. Christison alone recognized the fever of 1843 as identical with the synocha observed in 1819, and at that time treated (as Dr. Murchison has clearly shown) by copious venesection, with the supposed result of cutting it short at the close of the first week; a strictly normal crisis, however, of the fever itself (as observed in 1843), and quite erroneously attributed to the remedy.

But it is not with the depleting practices in epidemic fevers that we have to do, in the few minutes more that I claim your attention; but rather with the reaction against these, which in Edinburgh we always associated with the venerated name of Alison, and which in Dublin was no less characteristically set forth by a man of very different genius, but equally earnest, and equally devoted to his art—that great Irish physician who has left us his testimony in three notable words which, he said, might be inscribed upon his tombstone—"He fed fevers." It is not in this city that the practice of Dr. Graves, especially in typhus fever, requires to be explained or commented on, or even brought to mind as an historical fact; but I think this famous phrase was perhaps the first distinct recognition in Ireland of what is now one of the common-places of medical doctrine—that febrile disease of all kinds, being essentially a wasting process, requires to be met by such treatment as will preserve, rather than exhaust, what remains of the powers of life, and especially the greatly weakened functions of alimentation, digestion, and textural nutrition. Nor is it necessary to suppose that either Graves or Alison had anticipated, what came about in course of time, the extension of this general doctrine to nearly the whole field of disease, and the disappearance under its influence of a host of depleting and debilitating practices. But what is quite clear is that in typhus fever, as it occurred epidemically some time between 1819 and (say) 1829,[1] the idea first took root in some of the most prominent minds in the medical profession that alimentation, not depletion, was the proper practice to be pursued in most of the epidemic fevers. In Dr. Alison's case this idea was practically determined, to some extent, by his favourite notion of a "change of type;" and when "synocha" reappeared in 1843, it might, perhaps, have been expected, according to his theory, that he would have reverted to the old practices; but I have no evidence and no recollection of his having done so. In typhus, however, his practice was undoubtedly, and emphatically, one of stimulation; leaning, perhaps (though I am by no means so sure of this), more to the side of alcoholic stimulants, and less to that of food, than the practice of Dr. Graves. But in any case, the practice of both was guided by the same essential principle; that of supporting the strength, and so warding off the tendency to death by asthenia. This, too, he used himself to expound to us as really quite in accordance with the Cullenian tradition; inasmuch as Cullen's principle was, in fevers, to "obviate the tendency to death," rather than to adopt any violent or perturbative treatment.

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[1] The former date is that of the epidemic chronicled in Dr. Welsh's book; the almost indiscriminate use of the lancet being the leading feature of the practice as described at that time. The latter, is the period usually assigned by Dr. Alison himself as the limit in time to the appearance, within his own experience, of fevers that would bear depletion; and the probable date, therefore, of the more decided adoption of the opposite practice. See also note, p. 154.

But while the general plan or idea of supporting the strength in epidemic fevers may be said to have had its origin thus early, the modes in detail of carrying out that principle must have varied almost indefinitely; and it is probably quite safe to say that the general principle of Dr. Graves, to "feed fevers," became gradually merged, with some, into a process of high alcoholic stimulation as a substitute for feeding. It is very difficult to say how far individuals were responsible for the advance and development of this tendency in London, Dublin, or Edinburgh respectively; but the name of one very eminent Irishman who had attained a reputation of the highest kind in London, began, twenty years later, to attract attention as representing a kind and degree of stimulation, not only in fevers, but in almost all acute diseases, which had previously been very unusual, and had probably never before been advocated so ably, or in such seductive and uncompromising language. You all know well the name of this most able compatriot of Graves and Stokes; but as I have been accused by one at least of his countrymen of disrespect for his memory in submitting his opinions to professional criticism, even in the most carefully-guarded language, I will avoid as far as possible any needless allusions to him in this address, which may possibly find its way into other than medical hands. The theory adopted, however, was that alcohol is not only a real food, but that it is the food of all others—perhaps, indeed, the only food—to be relied upon for supplying the material of repair in fevers; thus compensating the waste and averting the functional exhaustion characteristic of most acute diseases. The practice associated with this theory was the subordination of all other nourishment (with the possible exception of beef-tea) to alcoholic liquors, and especially to brandy given in high doses continuously from half-hour to half-hour all day and all night, during the currency of severe cases of acute disease, and almost without any exceptions. So little, indeed, did the previous habits of the patient, the age, or the sex, count in this administration of alcohol, that a nursemaid, aged 18, had forty-eight ounces of brandy *per diem* at the height of her disease, and between four and five gallons in all; a rheumatic girl had six drachms of brandy every hour; and another girl, aged 14, far gone in organic disease, had several gallons of brandy in the course of a few weeks. These practices were, no doubt, conscientiously pursued and conscientiously defended; but the practical result of them was such a widespread and apparently irresistible professional bias towards the prescription of alcoholic liquors, especially in England and in the metropolis, that some of the leading physicians both in London and in the provinces, some years afterwards, found it necessary by a distinct manifesto,[1] which was largely signed, to dissociate themselves formally from an excess which in their deliberate opinion tended very decidedly both to moral and to physical evil, and made the profession at large appear to be partakers in the blame justly due to those who needlessly or recklessly foster habits of intemperance.

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[1] The history of this famous Declaration, as well as the substance of it, and some of the signatures to it in detail, will be found in the *British Medical Journal* for December 23rd, 1871, p. 737 ; and a more complete list of the signatures is given in the *Medical Times and Gazette* of the same date, p. 778. The opposite side of the question, professionally, in so far as it appears in the remonstrances of some of those who objected to this Declaration, will be found in the *British Medical Journal* for December 30th, p. 788 ; *Lancet* of same date, p. 931 ; *Medical Times and Gazette*, December 23rd (all of 1871), p. 779.

More than seven years before this point had been reached, however, in the opinion of these eminent English physicians, it had been shown in a Scottish hospital that in the one principal disease in which the entire medical tradition was unequivocally in favour of stimulation—namely, typhus fever—success was to be attained in treatment, not by increasing, but by very considerably reducing, the amount of alcoholic stimulants administered; and this, as compared with an amount which, even at its maximum, fell far short of that which was habitually prescribed in London. This inquiry was brought about, in the first instance, not by any *a priori* considerations, and still less by any fixed opinion adverse to the medicinal use of alcoholic stimulants, but by the fact that the author of it felt himself under an obligation to justify to one of his senior colleagues the mode of employing these stimulants at which he had arrived as the result of personal experience, and, in doing so, was compelled to investigate as carefully as possible, and as far as possible numerically, the whole of the data available in that particular epidemic and in that particular hospital. The results of this inquiry were communicated to *The Lancet* in March, 1864;[1] and this was followed up by several other papers, and particularly by one on "The Limits of Alcoholic Stimulation in Acute Disease," read to this Association at Oxford in August, 1868.[2] It would be vain to attempt here, at the close of this long address, even the most imperfect statement of these results. The one thing about them is that they have been accepted without having ever been seriously assailed. It may now be regarded as a position established by evidence that in typhus fever, and therefore *a fortiori* in other fevers and in all diseases attended by febrile debility, alcoholic stimulants are not the only nor yet the best form of alimentation; that their proper use is as medicines, and this within limits; and that, as a rule, and also within such limits as are suggested by reason and easily attainable in practice, milk (which during the alcoholic fanaticism had been nearly lost sight of) is in reality the food which is for the most part more easily digested and more conducive to success than almost any other. And thus the epigrammatic motto of your University Orator, "*Lacte, non vino,*" has received, I trust, at once its seal and its justification.

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[1] See *Lancet*, March 12th, 1864, p. 291, "Facts and Conclusions as to the use of Alcoholic Stimulants in Typhus Fever;" and also leading article, March 26th, 1864, p. 357. The other papers here referred to in *The Lancet* are as follows:—"Clinical Observations on the Course of Typhus Fever, and especially on the Phenomena of the Crisis," January 21st, 1865, p. 57 (remarks on Milk as a Food, p. 58), and "On the Treatment of the Acute Diseases of the Chest," January 13th, 1866, p. 29, and January 20th, p. 59 (especially the latter paper). Several of these papers are reprinted in this volume.

[2] *Glasgow Medical Journal*, November, 1868, p. 15, and *infra*.

I have thus endeavoured to show you, by a few instances only, in what manner, and on what lines, the art of medicine has been advancing towards stability, as I affirmed in an earlier part of this address. The last example is curious, because the Brunonian system, while theoretically very widely different, was characterized in some respects by a like excess in the administration of alcoholic stimulants to that which was current in London at the time just referred to. But no heresy like that of John Brown, crude and devoid of all foundation in real science, could have held the field for a single year in the middle of the nineteenth century. What has been doing in all these years has not been in vain. A vast army of earnest, unprejudiced, and disciplined workers has been engaged upon all the unsolved problems connected with disease. Diagnosis and pathology have made immense strides in advance during nearly the whole of the present century. Operative surgery has achieved the most brilliant and unquestionable triumphs of any period in the long course of medical history, and this in almost every department. Hygiene and sanitation, with their reflected influence on curative medicine, have advanced by leaps and bounds; and, even in the application of strictly therapeutic methods, the changes, though not so easily stated or so palpably appealing to the judgment of public opinion (inasmuch as quackery always is and always will be more self-asserting than truth) are all in the right direction. Polypharmacy, in its old and irrational forms, is now nearly gone. Hardly any one of intelligence supposes now-a-days that a disease is to be slain by a mere prescription or Latin formula. A science of pharmacology,[1] or of the uses and actions of medicines, is being built up upon a foundation that was never even possible before the present day, and which, beyond all question, will render this, the least satisfactory department of the science of healing, far more secure in the future. The mere simplification in the forms of remedial agents, and the mitigation accordingly of the old, inexpressibly repulsive, and nauseous manner of prescribing them wholesale (which incurred the ridicule of Molière two centuries ago, and yet lived on almost in our own day) represents an advance by no means devoid of importance as regards the comfort of the individual patient, and the peace of our families, and especially of our nurseries.

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[1] See, on this subject, the account of the unfulfilled aspirations of the late Sir Robert Christison, *Life*, vol. ii. p. 127, *et seq.* Also his *Address in Therapeutics*, read before this Association in 1858, and reported at length at the time in the *British Medical Journal.*

Nor is the immense relief to human suffering by the use of anæsthetics, rightfully employed, and of narcotics and anodynes in greatly improved forms of administration, to be lost sight of, even in the most brief allusion to the modern use of drugs.

All that removal, moreover, of the old traditional dogmatisms to which I have referred has been merely a clearing of the ground, so to speak, on which a modern science of healing, not by any means forgetful of its past history, but entirely unshackled by its old errors and prejudices, is being slowly but surely built up. And the fervid youth of the present generation, if they remain true to the spirit and not to the mere letter of the Hippocratic medicine, may well be grateful to their fathers and their grandfathers, were it for nothing else than this important and necessary process, whereby a soil has been prepared, at much cost of individual labour and sacrifice, on which they are permitted and privileged to sow the seed, and to reap more or less fruitful crops of good for humanity at large.

III.—FACTS AND CONCLUSIONS AS TO THE USE OF ALCOHOLIC STIMULANTS IN TYPHUS FEVER.[1]

[Prefatory Note.—The object of reprinting, in this volume, and in the order of their dates, this and the following papers connected with some important changes suggested in the treatment of acute diseases in 1864 and succeeding years, is two-fold. In the first place, the method and history of these changes are connected, in a way previously explained, with the general subject of the Address in Medicine, which forms the preceding chapter. In the second place, the temptation to a further expansion of the subject, at one time so strong as to have led me a certain length in the preparation of a separate volume on alcoholic stimulation in acute disease, was restrained partly by considerations which it is not, perhaps, necessary to set forth here, and partly by an indisposition, which as a physician I have all along felt, to be claimed as a partisan on either side of the so-called temperance, or total abstinence, question. The following pages have, nevertheless, been very largely quoted and insisted upon, not unnaturally, in temperance journals and other publications, as being more favourable to their side of the question than to the opposite; and, while I have neither the desire nor the power to interfere with the appropriation of my strictly clinical data as lending support to any more general hypothesis, it seems expedient, once for all, that the precise data themselves, together with such conclusions as appeared to me to emerge from them after many years' consideration of the whole subject in discussion, should be finally submitted to the public within the limits of a single volume. The leading article in *The Lancet*, which immediately follows this chapter, is instructive as showing forth editorially the drift of medical opinion in London at the time when my "Facts and Conclusions" were submitted and accepted by that journal as a necessary corrective; and I believe, accordingly, that without anything that can be seriously called personal controversy, a modifying influence has been exerted on medical opinion which will be generally recognized as beneficial, and to secure the permanence of which it is only necessary to have the facts in a sufficiently accessible form. I may add that my own attention was first called, not indeed to the subject itself, but to the need for dealing with it in this particular way, by the fact that my own practice as regards the treatment of epidemic fevers, in the Royal Infirmary of Glasgow, was privately called in question by one of my colleagues, long since deceased, but at that time occupying a position of seniority which I considered as entitling his criticisms to much consideration.]

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[1] This paper was first read to the Medico-Chirurgical Society of Glasgow, on 5th January, 1864, and afterwards published, with a few additions, in *The Lancet* of 12th March, 1864.

It is well known that for many years past there has been observed in medical practice, not only a tendency to a more liberal dietetic scale than formerly in almost all diseases, but also, and as a part of the reaction against the depleting and "lowering" system formerly in vogue, a very much increased allowance of alcoholic stimulants, especially in acute and febrile disorders. It is, perhaps, scarcely worth while to raise the question how far the medical sect of the so-called Brunonians may be entitled to the credit of having introduced this latter practice; for the influence of that sect died out, in this country, long before the habits of the present, or even of the last, generation of practitioners were formed, and the intervening period of habitual depletion has left its traces on medical practice too deeply to have escaped the observation even of some of the younger men of the present day. Of late years, the name of Dr. Todd has been more often heard than any other as the champion of high doses of alcoholic stimulants in acute disease. But my impression is that Dr. Todd was rather the expositor of a general tendency in medical practice than the deviser of anything really new and striking in theory in this matter. He was borne—willingly borne, it may be—upon a tide of opinion which, no doubt, he justified by more plausible arguments than most others; but he was in no sense whatever the originator of the doctrine or of the practice he followed with respect to alcoholic stimulants in acute disease. The marked approval with which his views were accepted in some quarters was the indication of their being received as the vindication of pre-existing opinions and practices, rather than as the assertion of a new principle; and it is known to most well-informed practitioners that, long before Dr. Todd adopted the routine of stimulation which he afterwards advocated, similar practices had been widely diffused throughout the profession, both in the metropolis and elsewhere. In particular, as regards typhus fever, doctrines not very dissimilar from those of Dr. Todd were taught as early as 1826 by my much-revered teacher, the late Dr. Alison; with this difference, however, from the doctrines and practices of Dr. Todd, that the latter physician was both more extreme and more indiscriminate in his use of stimulants than the former, Dr. Alison relying upon a specific *observed* change of type in disease as the justification of his practice, while Dr. Todd, on the other hand, considered the doctrine as superfluous, if not wholly unfounded, and unhesitatingly advocated the lavish use of stimulants in almost all acute diseases, and in almost all stages of them, with but little regard to individual differences of age, sex, etc., and without any attempt to guide his administration of wine and brandy by the study of the peculiarities of the epidemic, or the previous habits of the individual patient.[1] I believe, however, that all the old pupils of Dr. Alison will recollect his constant and anxious instruction to the effect that, in typhus fever, as well as in other diseases of a typhoid character, stimulants were always to be administered from an early period, without waiting for symptoms of debility, or for any other special indication, except the ascertained nature and tendencies of the disease; and, in point of fact, the administration of wine and spirits by Dr. Alison, in such diseases, must be well remembered by all who observed it, as being practically of the nature of a routine, not, indeed, nearly so extreme or unqualified as that of Dr. Todd, but essentially of the same character; the use of stimulants being commenced almost invariably before the end of the first week, and sometimes as early as the second or third day, and continued throughout the disease in liberal doses, increased according to the violence of the symptoms. There can be no doubt that this practice of Dr. Alison, carefully followed out by his pupils, Drs. Graves and Stokes in Ireland, and by others all over the world, had a powerful influence in breaking up the old bad routine of depletion in typhus fever, which was almost universal in the earlier part of the present century; but it seems to me hardly less probable that to the exaggeration of Dr. Alison's rules of practice we owe, indirectly, the more extreme doctrines of these latter days as regards the use of stimulants in almost all acute diseases. But be this as it may, I propose, in the present paper, to avoid all disquisitions of a theoretical kind, and to allow my own opinions to emerge from a simple statement of facts, which will, I trust, be considered not unworthy of attention, even should the inferences I draw from them be regarded as not fully proved.

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[1] I infer this from the fact that some of the most considerable doses noticed in Dr. Todd's book are given in cases of young women—*e.g.*, to a girl nineteen years of age, six drachms of brandy every hour (Case 64); and this without any statement in regard to previous habits.

There are many reasons for raising the question of the practical limits of the administration of stimulants, in the first instance in the case of typhus fever. For, whatever differences of opinion may exist on the subject in general, no one can refuse to admit that typhus fever is the disease of all others in which the practice of stimulation has been most largely followed, and in which it has found the most general acceptance, as being in accordance both with rational theory and actual experience of facts. Moreover, typhus fever, as marked by the eruption, is a remarkably well-defined disease, in which, with proper securities against error, there is likely to be but little difference between the diagnosis of one well-informed hospital physician and another; and besides, typhus has constituted by far the largest proportion of all the fevers treated in hospital in Glasgow of late years. Further, the arrangements of the Glasgow Fever Hospital allow of a tolerably accurate estimate both of the usual mortality of typhus fever, and of the amount of stimulants habitually administered. Finally, it is very satisfactory to myself that this can be done without involving personal considerations, or placing my own practice in opposition to that of any other individual physician. For all these reasons, I propose, in the present paper, to confine the argument rigidly within the bounds assigned to it in the title, although it would be easy to strengthen my case by allusions to other diseases, and to other fields of practice than the hospital.

I expect, then, to be able to show in this paper—1st. That it is possible to reduce the mortality of typhus fever, while withholding a large proportion of the amount of alcoholic stimulants usually given. 2ndly. That this diminution of mortality may take place at all ages, but is most marked amongst the young. 3rdly. That while at all ages the administration of stimulants ought to be very strictly guarded, as likely to prove injurious when in excess, it is demonstrable that young and temperate persons may be advantageously treated —*i.e.*, treated with a diminished mortality—*without one drop of wine or spirits being given from beginning to end of the fever, except in the rarest casualties.* 4thly. That the principle of giving stimulants *as a matter of routine*, in typhus—*i.e.*, at a certain stage of the disease, with but little regard to individual peculiarities—ought to be at once abandoned. 5thly. That an approximation can be made to a more correct doctrine on the subject, though further researches are still required. 6thly. That there is reason to think that in most hospitals, as well as in private practice, a very large needless expenditure is incurred through neglect of these facts—a consideration which, though secondary in importance to others, is by no means to be set aside as irrelevant.

The facts from which I propose to attempt to prove these positions are—1, the cases treated by myself, or under my immediate superintendence, in the Glasgow Fever Hospital, since the middle of November, 1862; 2, the entire aggregate of cases treated in the years 1861 and 1862, these years being selected merely as being the nearest in point of time to my own appointment as physician, though only to a very slight extent (in the end of 1862) influenced by my own practice, and therefore in all probability fairly representative of the existing state of opinion and practice in Glasgow. I will add, that nothing in my experience leads me to believe that the practice in Glasgow is materially different from that usual in Edinburgh, or in Scotland generally, in this respect; while I am fully convinced that the tendency to what I regard as an excess is far greater in London, and probably in many parts of England, than it is in either Edinburgh or Glasgow.

The cases treated by myself are roughly divided into three series in the order of time. Although the numbers in these series are probably too small, separately considered, to give a secure basis for numerical analysis, I have thought it desirable to preserve the distinction, partly because it illustrates a very remarkable increase in the mortality of the fever in the middle period (April to June, 1863), and partly because the returns were carefully completed in private memoranda left me by my dear friend and fellow-labourer, the late Dr. Hamilton,[1] whose early death, from this very disease, typhus fever, has saddened all who knew him as with the sense of an irreparable loss. To his earnest love of his profession, to his simplicity and truthfulness, not less than to his great quickness in receiving and applying new ideas without prejudice, I am more indebted than I can possibly express; and without his efficient assistance it is very certain that the facts in this paper could not have been presented in a satisfactory form. The basis thus obtained was further extended, and even improved upon, by his successor and friend, Dr. Marshall,[2] my assistant in the wards from August to December, 1863. To Dr. Duncan, my present assistant,[3] I am also indebted for important aid in consulting the journals of the house; and I am entirely convinced he has carried out the analysis of these facts in a spirit of the most rigid impartiality, and with the most perfect accuracy that was possible under the circumstances.

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[1] Son of the late Rev. J. B. Hamilton of Kilmarnock. Dr. Hamilton was a young man of unusual accomplishments, and of the greatest promise and devotion to his work. He was my first resident assistant in the Glasgow Royal Infirmary, and to the great grief of all who knew him was cut off by the epidemic, while afterwards acting in the Royal Infirmary, Dundee.

[2] Now of Barnes, Surrey.

[3] Afterwards of Bradford, since deceased.

The returns thus prepared have been reduced to a commodious form in several tables, on which I will proceed to remark.

The first table exhibits the result of 595 cases of typhus fever occurring in regular succession between November, 1862, and December, 1863, with the exception of a few cases admitted in June and July, which Dr. Hamilton had not included in his return, as they occurred after he had made it up at my request, and before Dr. Marshall took his place. I have not thought it necessary to re-open the documents in order to include these cases, as I feel certain they would in no way have modified the conclusions to be drawn from the rest. The utmost care has been taken to include every case, even when the patient has died within an hour or two after admission, and before any treatment could be brought into operation. Every case recorded in the journals as typhus was the subject of a special diagnosis, either on the ground of a characteristic eruption, or of such a marked association with a group of eruptive cases as left no reasonable doubt of the nature of the disease. In point of fact, a very large proportion indeed of the cases were distinctly *eruptive* typhus, the absence of eruption having been quite an uncommon fact in my later experience of typhus. The experience of my colleagues may be confidently appealed to moreover, to show that there has been very little chance during the present epidemic of mistaking typhus for any other fever. The character of the epidemic has been exceedingly well marked, and no practical difficulty has arisen in classifying the cases. Nor is there any reason to suppose that the principles of diagnosis in 1861-62 differed materially from those employed by myself in 1862-63. The classification of the cases in Table II. contains ample proof of the great predominance of typhus over all the other forms of fever admitted in these years.

The aggregate mortality of the 595 cases referred to as admitted under my care was 71, or within a fraction of 12 per cent. (See Table I.) It is remarkable that for about two months (constituting the second series) the mortality rose to 19 per cent.; and I ascertained at the time that this rise in the rate of death was experienced all over the fever wards, as well as in the cases under my own care, being quite evidently dependent upon a graver character of cases admitted.

The mortality was, as usual, much greater among the adult and aged patients than among the young. Taking the age of 15 or 16 as the boundary line, it will be observed that among 406 cases above that age 70 died, or about 17·2 per cent.; while of the 189 cases below that age only *one* died, a child aged six years, admitted only to die. (See Table I., note.) It is to be remarked, however, that while Dr. Hamilton included the sixteenth year (*i.e.*, 15-16) in his returns, Dr. Marshall excluded it in the third series, which is to this extent, therefore, different from the others. It did not appear to me that the difference in this respect was of very material consequence as to the result; but it remains to be stated that had Dr. Marshall included the sixteenth year there would have been one more death to record in upwards of 190 cases. The total mortality, therefore, of the cases decidedly below puberty (even including the infant admitted moribund) is less than 1 per cent., and might be reduced to absolutely *nil* by excluding the deaths within twenty-four hours, according to the practice of many hospitals.

Comparing now these results with those of 1861 and 1862 (Table III.), we find that the mortality per cent. of all ages in typhus is stated as 18·86 and 16·92, the average of both years together being 17·69 per cent.; while the adults died in the proportion of about 24 per cent., and the *young* in the proportion of 3·2 per cent. in the former and 3·6 per cent. in the latter year. It need hardly be insisted on that the returns in Table I. present an advantage in point of mortality over those in Table III., at every period of life. Thus, for a mortality at all ages of under 12 per cent. in Table I., there is in Table III. 17½ per cent.; for a mortality among adults of 17 per cent. in Table I., there is 24 per cent. in Table III.; and for a mortality of upwards of 3 per cent. among the young in the latter table, the former gives only one death of a child admitted moribund, and one other of a girl above fifteen years of age, in a total of 190 cases or more. So far as these facts, therefore, can be admitted to be in any sense conclusive, they establish the position of a relatively low rate of mortality in the cases in Table I., both for the earlier and the later ages.[1]

Again, comparing the results in Table I. with those given in Dr. Murchison's tables, deduced from the large experience of the London Fever Hospital over fourteen years and a half, we find that in place of a mortality at the rate of less than 12 per cent. for all ages, the average rate in London has been no less than 20·89 per cent. I am, indeed, very far from wishing to indicate that this comparison is really a fair one, for I know well how much the mortality of fever in different hospitals is due to other causes than differences of management, and I believe that the London Fever Hospital does in fact receive a larger proportion of old and destitute persons than most others; but Dr. Murchison has carefully compiled statistics from a number of different places, Glasgow included, from which he arrives at the conclusion that the average mortality of typhus fever in hospitals, excluding local and accidental causes of difference, is not less than 18 per cent.[1] In the practice of the late Dr. Todd, in King's College Hospital, Dr. Murchison found the general mortality of typhus to have been about 25 per cent.; and in two only of the 108 cases on which this deduction is founded did the death take place within forty-eight hours after the patient's admission, showing that the circumstances of the removal to hospital were not, in even so great a degree as usual, responsible for this high rate of mortality.

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[1] The number of males and females in my wards being very nearly equal, I have not thought it necessary to make any subdivision of the returns according to sex.

The preceding results may be briefly summed up thus:—

	Per cent.
Rate of mortality of 595 cases in Table I., without selection and without exclusion,	11·93
Average rate in London Fever Hospital,[2]	20·89
The same, with exclusion of cases dying in less than 48 hours,[2]	17·94
Rates in Edinburgh epidemic[2] of 1847-8,	20·6 to 24·72
In Barony Parish Hospital, Glasgow, in the same epidemic,[2]	17·23

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[1] *Treatise on Continued Fever*, 1862, p. 218. I think it desirable here to guard against the conclusion, which might possibly be inferred from the reference to these averages in this paper, that the practice either in London or Glasgow has been throughout upon a high scale of stimulation. I wish these numbers to be received merely as the result of the largest available collection of data for estimating the habitual mortality of typhus fever in hospital practice, under every variety of treatment.

[2] Murchison, p. 217-18.

	Per cent.
In Glasgow Infirmary, 1843-53,[1]	18·00
In the same, 1857-61,[1]	16·83
In the same, 1861 (Table III.),	18·86
In the same, 1862 (Table III.),	16·92
In King's College Hospital, under Dr. Todd (approximative),[2]	25·00

Taking the cases of the young alone, as opposed to the adult population:—

Rate of mortality of 189 unselected cases in Table I. (inappreciably small, but say),	1·0
Rate in Glasgow Fever Hospital, 1861,	3·2
Do. do., 1862,	3·6
Do. do., 1847,[3]	5·0
Rate in London Fever Hospital during 10 years,[4]	6·2
Rate in King's College Hospital, under Dr. Todd,[5] possibly about	17·0

[1] Murchison, 217-218.

[2] *British and Foreign Med. Chir. Review*, October, 1860, p. 332. Read also the controversy in *The Lancet*, October and November, 1860.

[3] 163 cases below 15 years of age, 3 deaths. Dr. Steele, in *Edin. Med. and Surg. Journal*, vol. lxx., 1848.

[4] 563 cases below 15 years of age, 35 deaths. Murchison, p. 22, Table XV.

[5] See data in *British and Foreign Med. Chir. Review*, as above, The numbers, indeed, are too small to justify a precise statement of the rate of mortality : but it is stated by Dr. Murchison that

On the other hand, taking the adults alone, the differences in the rates of mortality are, of course, much less striking, but are still in favour of Table I., which gives an average rate of 17·2 per cent., as compared with 24 per cent. in the Glasgow Fever Hospital in 1861-62.

I have now to refer to Table IV., in evidence that these very favourable results have been attained with a much smaller amount of stimulation than has been usual of late in Glasgow, or even in my own previous practice in the Edinburgh Infirmary. The figures in the table, indeed, speak so plainly upon this point that it would scarcely be necessary to make any remarks upon them, were it not that the subject is one of such vast importance, and that all numerical data, unexplained, are liable to a certain extent to misconstruction. I desire, then, that it should be very clearly understood that there is no evidence whatever in these numerical data of anything that can be called special profusion in the use of alcoholic stimulants in Glasgow as compared with other places. On the contrary, I have a strong conviction based upon facts which, though not capable of being numerically stated, leave

of 34 cases below 20 years of age, 6 died, or at the rate of 17·64 per cent. From the table in p. 221 of Dr. Murchison's book, it further appears that the rate of mortality in typhus below the age of 15 is in London even higher than that of all the cases below 20. I wish, however, these statements to be taken as approximative only.

me in little doubt personally that both in London and Edinburgh (but especially in the former) the habitual administration of alcoholic stimulants in hospital practice has been at a higher rate than in Glasgow; and I am also well assured, from detailed examination of the facts, that my own rate of administration in Edinburgh, even when at the highest, was decidedly less than that of some of my colleagues. On the other hand, the comparative moderation of the Glasgow annual averages leaves room for considerable individual varieties of practice; and here, too, it is quite plain to me that the excesses of some physicians are compensated in a great degree by the moderation of others.

With these explanatory remarks, I would now direct special attention to the averages of the third series of my own cases, as recorded in Table IV. It is possible, indeed, that this average may in the end turn out to be practically *too low* to meet the requirements of the severest types of epidemic typhus; but I am very sure that the cases to which these numbers apply were of quite average severity, according to my previous experience of typhus fever. It is true that during several years in Edinburgh, in which my numerical averages show a greater consumption of alcoholic liquors, the fever was much milder, and even less fatal than in Glasgow in 1862-63; but I have now a most assured conviction, founded on careful observation of details which can hardly be stated here, but have been referred to elsewhere,[1] that the fever of that period was quite *exceptionally* mild; and I am now equally convinced that to whatever extent I gave stimulants greatly in excess of my present practice at the period referred to, they were misapplied and wasted, if not indeed in part ordered and used without my knowledge of the fact.[2] At all events, it is quite certain that in typhus fever by no means exceptionally mild, treated with a daily average of spirits not much more than half, and of wine less than the eleventh part of the quantity administered over the entire fever hospital in 1861-62, a rate of mortality was attained of not more than 10 per cent. Or, to put the result into the form of larger figures, the facts are as follows:—Between August 3rd and December 7th, 1863, there were treated 135 males and 134 females affected with typhus fever, being a total of 269 cases treated to a termination in my wards. Of these cases 27 died, being at the rate of 10 per cent. During the whole course of the treatment the 269 cases consumed 633 ozs. of port wine and 666 ozs. of whisky, or at the rate of about 2¼ ozs. of wine and 2½ ozs. of spirits on an average to each patient,[1] as the allowance for his entire period of residence, which, on an average, was 20½ days.[2]

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[1] *Clinical Medicine*, 1863, p. 158.

[2] The reader of pp. 58-63 of my *Clinical Medicine* will have no difficulty in perceiving that a suspicion of this kind was present to my mind in writing a former paper upon this subject; and the renewed experience, in Glasgow, of a practice conducted upon nearly the same principles, but much more carefully watched in detail, convinces me that the apparent rise in the averages of alcoholic liquors from 1856 to 1860 was really due to a diminished personal control of the wine roll, rather than to an increased faith in the remedy.

On the other hand, in the Glasgow Fever Hospital, during the years 1861 and 1862 taken together, there were 1837 cases under treatment, although only 1694 of these were treated to a termination during the period assigned, and only 1289 were typhus. There were consumed in the two years 62,754 ozs. of wine, 8440 ozs. of whisky, and 2611 ozs. of brandy. Adding the two last sums together and dividing by the number of patients, we find that each patient consumed, on an average, more than 34 ozs. of wine and 6 ozs. of spirits. But, as much the largest proportion of this quantity was probably given to the 1289 cases of typhus, it is probable that the real average quantity consumed by the typhus cases may have been not less than 40 ozs. of wine and 7 ozs. of spirits, which would be nearly sixteen times as much wine and three times as much spirits as was given to the cases in the third series above referred to. With this much larger consumption of alcoholic stimulants, the mortality of typhus was no less than 17½ per cent. in these two years, while under the lesser amount of stimulants it was only 10 per cent.

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[1] A friend, to whom I showed this paper, inferred from the statement in the text that I was in the habit of administering both wine and spirits in the same case. It seems necessary, therefore, to guard against misconception by affirming that this mixture of stimulants was scarcely ever prescribed in the cases here reduced to general averages.

[2] It is possible that the short period of residence was due in part to the great pressure in the fever wards during some part of the autumn and winter months.

It will not fail to be apparent to all persons accustomed to the use of averages, but may, nevertheless, require explanation to some readers of this paper, that the figures in Table IV. give no true indication of the actual amount of stimulants usually prescribed in cases supposed to require them. The numbers show merely the most general results of treatment, and cannot be trusted for guidance in the details of particular cases. Thus, the low averages in my own cases of the third series undoubtedly depend much more on the fact, that at least nine tenths of those treated had no stimulants at all, than on the restricted quantities given to those who were considered to require stimulation. In fact, the small minority (including nearly all the fatal cases) who received stimulants, had them administered in very considerable quantities. A like statement, of course, applies to all the other averages recorded in Table IV., with the difference that in some of the cases referred to, the proportion treated without stimulants was much lower than in my cases.[1]

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[1] In this paragraph, as here reprinted, a few sentences are omitted which, as originally published in *The Lancet*, appear to be open to question as regards the details, though not at all as regards the general principle involved.

It is plainly inexpedient to attempt to establish any fixed rule for the administration of stimulants upon a numerical basis, and it may be safely asserted that such a rule, if established by authority, would be set up only to be broken. But it is a consideration of no small importance, that if it should appear hereafter that typhus can be usually treated with an amount of stimulants nearly such as I have here indicated, the saving to our hospitals would be about a bottle and a half of wine, and more than three ounces of spirits, in each case of typhus fever. I do not wish, however, to press this view of the subject, which is plainly of subordinate importance to the question of a sound and successful treatment.

Having regard to the fallacies inherent in all very dogmatic statements upon a question such as that of the treatment of typhus fever by alcoholic stimulants, I am content if the reader follows me on this occasion so far as to admit that a fair presumption has been established in favour of a considerable reduction in the quantities at present habitually employed. In other words, I hold it as proved that such a reduction may be effected without any detriment to the safety and success of the treatment; and further, I regard it as very probable, that the general mortality of typhus will be lessened exactly in proportion as the routine of a vicious and excessive stimulation is abandoned in favour of a judicious and careful system of treatment by all the other means known to physicians. But in regard to the case of young persons, and especially persons below the age of puberty, when affected with typhus, I think the facts before me warrant a much stronger and firmer position than this. I have long been convinced, in fact, by carefully-watched experience of individual cases, that in persons of immature age, and of temperate habits, stimulants should very rarely be administered, except in minute and experimental doses, rather as tonics than as stimulants in the proper sense of the term; and, possibly, in some very extreme cases of collapse, in more considerable quantities; but not even then with any approach to intoxicating doses. In typhus fever, I am now well assured that this view is well founded, for I have to record the fact, that the treatment which gave only one fatal case in 189 young persons was a treatment from which stimulants were practically excluded, the only patient who had a little wine being a child with cancrum oris, with great debility of the stomach, long after the fever had run its course. Looking at the results of this practice beside some of those formerly indicated, and especially looking to the rise in the rate of mortality amongst the young in typhus fever in proportion as there seems reason to believe that the practice has leaned to a routine of stimulation, I confess I am strongly persuaded that *to the young, in typhus, and very probably in most other fevers, stimulants are not less than actively poisonous and* *destructive,* unless administered with the most extreme caution, and in the most special and critical circumstances. Had my 189 cases of young persons died at the rate even of those in the Glasgow Fever Hospital in 1861 and 1862, the deaths, instead of one, would have been six or seven; had they died at the rate of the Glasgow epidemic of 1847, which, however, I admit to have been a more severe epidemic than the present, the mortality would not have been less than nine; at the rate of the London Fever Hospital it would have been nearly twelve; while, in the hands of Dr. Todd, under a routine of such extreme stimulation as is indicated in his book on acute diseases, it seems probable that instead of *one death* in the 189 cases, there must have been no fewer than thirty to thirty-five!

I trust it will not be supposed from anything I have written above, that the views which I wish to suggest have anything in common with those of the *depletion and starvation* school of physicians, or such as condemn the use of stimulants, *in toto,* in fevers, from the idea that they tend to "excite the circulation." With this school of physic neither my own personal opinions, nor my actual practice, have anything in common. I do not deprecate the excessive use of stimulants on account of their tendency to "excite the circulation," nor do I advocate the use of depletion or of starvation as a corollary to the doctrine of lessened stimulation; on the contrary, the whole aim of the practice I adopt in fevers is what is commonly called " supporting the strength;" and the objection that I entertain to the use of stimulants in excess, or upon the plan systematically advocated by the late Dr. Todd, is, that so given, they do not support the strength, but rather poison the system, by loading the blood with material which is inconvertible into blood or tissue, and which arrests or interferes with the vital changes necessary to preserve the balance of waste and supply. The essential questions, however, connected with this subject, are foreign to the plan of the present communication, and I only advert to them in order to draw a sharp line of distinction between the objections I entertain to excessive stimulation in fevers, and the practice so common a quarter of a century ago, of encountering the febrile excitement by depressing remedies, and by starvation or extreme restriction of the diet. I can well believe, therefore, that Dr. Alison, and after him Drs. Graves and Stokes, in introducing a " supporting " practice in fevers in opposition to the depleting practice formerly followed, initiated a great improvement, and saved the lives of a great many of their patients. The only question with me is how to secure the advantages of a " supporting" practice, without at the same time running a risk of poisoning the blood with repeated doses of alcohol, given without due observation of the effect of each dose, and so as to interfere with the real nutrition of the system, rather than to promote it. That all routine methods of administration tend to this abuse I have satisfied myself by careful observation both of my own practice and that of others whom I have met in consultation and otherwise; and believing, as I do, that such a tendency to excess exists, I venture to ask the assistance of my medical brethren towards a fair and calm consideration of this great question.

TABLES IN ILLUSTRATION OF THE PRECEDING PAPER.

I.—Abstract of Cases, Deaths, and Mortality per cent. in Typhus, in cases treated by Dr. Gairdner, in Glasgow Fever Hospital, 1862-63.

CASES.

	All Ages.	Young.	Adult.
1st Series, -	225	72	153
2nd do., -	101	31	70
3rd do., -	269	86	183
All three, -	595	189	406

DEATHS.

	All Ages.	Young.	Adult.
1st Series, -	25	0	25
2nd do., -	19	0	19
3rd do., -	27	1[1]	26
	71	1	70

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[1] The one death was of a child aged 6, admitted moribund, and not seen by Dr. Gairdner, nor placed under treatment. Another

MORTALITY PER CENT. (APPROXIMATIVE).

	All Ages.	Young.	Adult.
1st Series, -	11·1	—	16·3
2nd do., -	19·0	—	27·1
3rd do., -	10·0	?	14·3
All three, -	11·9	1 ?	17·2

II.—Abstract of Cases treated in Glasgow Fever Hospital during the two years 1861 and 1862; showing also the average period of residence (in days), and the total consumption of alcoholic stimulants (in ounces), during each year.

	1861.	1862.
Total under treatment, - -	790	1,047
Treated to a termination, - -	709	985
Typhus, - - - - -	509	780
Typhoid, - - - - -	36	79
Variola, - - - - -	73	43
Febricula, - - - -	54	29
Pneumonia, - - - -		18
Scarlatina, - - - -		10
Influenza, - - - -		11
Average period of residence (in days),	28	27
Total consumption of whisky (in ounces),	3,820	4,620

death, however, occurred during the 16th year, in a girl from Coatbridge; and since the return was made up as above, a death has occurred in a girl of six months, who had typhus after variola, and was admitted in a very low state.

	1861.	1862.
Total consumption of brandy (in ounces),	449	2,162
Do. of wine (in ounces),	29,272	33,482

III.—Abstract of Cases, Deaths, and Mortality per cent. in cases registered as Typhus (see Table II.) in Glasgow Fever Hospital.

CASES.

	All Ages.	Young.	Adult.
Year 1861, -	509	154	355
„ 1862, -	780	247	533
Two Years,	1289	401	888

DEATHS.

	All Ages.	Young.	Adult.
Year 1861, -	96	5	91
„ 1862, -	132	9	123
Two Years,	228	14[1]	214

MORTALITY PER CENT.

	All Ages.	Young.	Adult.
Year 1861, -	18·6	3·2	25·6
„ 1862, -	16·92	3·6	23·1
Two Years,	17·69	3·4	24·1

[1] The ages of these fatal cases were as follows : at 4 years, 1 case ; at 6 years, 1 case ; at 7 years, 2 ; at 9 years, 2 ; at 10 years, 1 ; at 11 years, 3 ; at 12 years, 1 ; at 13 years, 1 ; at 14 years, 2.

IV.—Averages of Wine and Spirits employed in Glasgow and Edinburgh Fever Wards, showing the quantity (in ounces) ordered for each patient in twenty-four hours, at the periods and under the circumstances mentioned below.

——	Ounces per Patient ordered in 24 hours.	
	Wine.	Spirits.
Average of five years in Edinburgh (Dr. G.), -	1·314	0·137
Maximum—Average of year 1858 (Dr. G.), -	1·734	0·346
Minimum—Average of year 1856 (Dr. G.), -	0·715	0·069
1st and 2nd Series (Table I.), (Dr. G.), - -	0·261	0·214
3rd Series (Table I.), (Dr. G.), - - - -	0·115	0·121
Two years in Glasgow Fever Hospital (Table III.), (various Physicians), - - -	1·304	0·229

Leading Article from "The Lancet," March 26th, 1864, *in reference to the above.*—[A very important clinical paper, by Dr. Gairdner, of Glasgow, appeared in *The Lancet* of the 12th instant. It treats of a question in practical medicine, which, to all thoughtful minds in the profession, must seem one of the most urgent medical questions of the day—namely, the use of alcoholic stimulants in disease. As our principal object in this article is not so much to discuss the question as to direct attention to Dr. Gairdner's paper on it, we will state the facts which seem to us to give this communication a great importance.

Dr. Gairdner is, by general consent, regarded as one of the most promising of the present generation of physicians in Scotland. He speaks from experience of vitality and disease in large cities and on a large scale. Formerly connected with the Edinburgh Infirmary, and latterly with the Glasgow Infirmary, he is entitled to speak authoritatively. But there is another feature of Dr. Gairdner's writings which must always command respect, and that is, his great moderation and care, both of statement and inference; the anxious way in which he qualifies conclusions which may seem favourable to his own views, or against the views and practices of others. There has been an unseemly extremeness in the doctrines of leading members of the profession of late years, represented by a lingering faith in bleeding on the one hand, and a newly-acquired belief in brandy on the other. Upon such men as Dr. Gairdner will devolve the duty of mediating between the extreme views of the past and the perhaps equally extreme views of the present. We are convinced that the truth lies in neither extreme, but somewhere in the middle, and the sooner it is discovered and applied the better for the credit of medical practice. The paper treats of the use of alcohol in typhus fever, the disease in which, of all others, it has been considered most important.

Three different degrees of stimulation may be considered as discussed by Dr. Gairdner. Firstly, that minimum degree of stimulation practised by himself; *at least nine tenths of the patients having no stimulants at all*, the whole quantity consumed giving, as an average to each patient, during 20½ days, the average period of residence, two ounces and a quarter of wine and two ounces and a half of whisky. Under this treatment, in 1861 and 1862, with a form of fever by no means unusually mild, the mortality was 10 per cent. It is important to notice that in all Dr. Gairdner's fatal cases a very considerable quantity of stimulants was given. Secondly, that degree of stimulation practised in the Glasgow Fever Hospital, which would give an average allowance to each patient with typhus of forty ounces of wine and seven ounces of spirit, and under which 17½ per cent. of the patients died. Thirdly, the extreme stimulation practised by Dr. Todd. Particulars are not given by Dr. Gairdner, but it is matter of notoriety, that the cost for wine and spirits in the London hospitals reaches, or did reach, its maximum in King's College Hospital, and that Dr. Todd, in particular, thought nothing of prescribing half an ounce, or even an ounce of brandy per hour in cases of fever. The mortality of Dr. Todd's cases of typhus, according to Dr. Murchison, is 25 per cent.

Excluding adult patients, there is a frightful difference in favour of the non-alcoholic treatment. Amongst the young cases treated by Dr. Gairdner, the rate of mortality of 189 unselected cases was inappreciably small, but say 1·0 per cent." The rate in Glasgow Fever Hospital generally was about 3 per cent. In King's College Hospital, under Dr. Todd, it was about 17·0 per cent. Dr. Gairdner thinks very strongly of the injuriousness of stimulants administered to fever patients of immature age; indeed, except in one such case, in which cancrum oris occurred, they were practically withheld. He calculates that if Dr. Todd's system had been practised in the treatment of his 189 young cases, instead of 1 death there would have been from 30 to 35.

There may be some grand fallacy lurking in these calculations of Dr. Gairdner, and alcohol, and lots of it, may, after all, be the panacea which, to judge from the medical fashion of the moment, it is held to be. If so, we shall soon have the fallacy pointed out, and the favourite remedy duly magnified; but we have long had a most disagreeable fear that the lavish prescription of alcoholic stimulants, in London practice especially, would one day be discovered to be a great medical error, only less than the opposite system which preceded it. It has been pushed amongst all classes of patients—rich and poor, women and children; in acute cases, in hospitals and in private practice, money has been most doubtfully squandered over bottles of wine that would have sent the patient once convalescent to the seaside, or that would have paid twice over many an unpaid bill. More serious still is the moral effect which this fashion of the faculty is calculated to have on the public. Still considerations of mere cost or morals in a life-and-death question are essentially secondary. If there are any physiological or therapeutical facts which can be regarded as amounting to a proof that alcoholic stimulants in large quantities have a saving or curative effect in fever, not to be got in any other way, they must be administered at all hazards and at all cost. This, however, is just the rub. Dr. Gairdner seems to suspect that alcohol tends "to poison the blood," and there is certainly much both in recent physiology and pathology to make us doubt whether, the blood being already poisoned, as it is in fever, alcohol is likely to contribute anything to its purification.

We do not wish to go into details here, or to dogmatize, or too hastily condemn a practice which, intelligibly enough, has found so much favour; but if the propriety of it is doubtful, there is nothing so proper as the criticism of it in these columns. The origin of the practice must be mainly attributed to Dr. Todd; but the responsibility of continuing it must rest with the profession in London, which, it is understood, has adopted Dr. Todd's views. We do not go so far as to say that a case has been already made out for abandoning entirely the administration of large quantities of stimulants in fever, but a most grave doubt is thrown upon the wisdom of the practice by Dr. Gairdner's statistics. We shall look most anxiously to the advocates of high stimulation for an explanation of the fact, that fever in Glasgow, under a very moderate allowance of wine and stimulants, kills only 10 per cent., while in London, under high stimulation, it kills 25 per cent. It will not avail to speak of the low physical condition of the people of London, for any difference in this respect between Glasgow and London must be in favour of the latter. It will not avail to speak of the typhoid type of disease, for it is in typhus itself that Dr. Gairdner has found the advantage of a moderate, or, to speak more accurately, an exceptional administration of stimulants; for, we repeat, it is only in exceptional cases that Dr. Gairdner gives stimulants at all. Indeed, a fanatical critic might find a striking correspondence between the percentage of deaths and the percentage of cases in which stimulants were administered, and a closer inspection of Dr. Gairdner's paper shows a more striking coincidence still, inasmuch as the 10 per cent. of his cases which got a good allowance of stimulants were the identical 10 per cent. which died. We do not construct the syllogism at which we hint out of these tempting materials. We can easily understand that it would be perfectly false, but we very seriously commend the whole subject to the attention of the profession, especially in London.]

Note on the Changes described as occurring in the Epidemic Fevers of Edinburgh, 1826-29.—I am indebted to the marvellously careful and thorough "Historical Account of Typhus Fever" in Dr. Murchison's great work, for two references, which, followed up with some detail while these pages are passing through the press, have enabled me to place before the reader the precise dates and circumstances of the change in practice in Edinburgh referred to above (pp. 113, *note;* 125) as having occurred in or about the years, 1826-1829. In October, 1828, an anonymous reviewer in the *Edinburgh Medical and Surgical Journal* (whom I am almost disposed from internal evidence to pronounce to have been Dr. Christison)[1] writes as follows in reference to a work on the *Typhus or Adynamic Fever*, recently published in London:—"We do not know whether he (the author) will be indebted to us for informing him that we should discover little to alter even in his description of the treatment; and that, notwithstanding his insinuations against the practice pursued at a 'celebrated University,' he will find our hospital practitioners are by no means ignorant . . . . . of the necessity of mitigating, in all the varieties of that form of fever (Typhus), the vigorous blood-letting which during his studies he saw employed in the epidemic of 1817-20, and which some . . . . . have been too apt to transfer to all succeeding epidemics here and elsewhere, whatever might be their epidemic character."

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[1] He tells us in his Autobiography (*Life*, vol. i. p. 370) that in 1827 he had "just become, along with Dr. Craigie, joint editor of the *Edinburgh Medical and Surgical Journal.*" Dr. Craigie's opinions on blood-letting, maintained up to a much later date, would probably have hindered his using some of the expressions quoted below. Dr. Christison, on the other hand, was already thoroughly familiar with the whole subject, had written an account of the epidemic of 1817-20, and was now an ordinary physician of the Royal Infirmary.

We have here contemporary evidence that in 1828 misgivings had begun to arise as to the employment of blood-letting in epidemic fever after the manner of Dr. Welsh; and also that, in London, the Edinburgh school was regarded by some as too much disposed to maintain the practice in its original severity. This accusation may have been (as the anonymous reviewer affirms) either unfounded or overstrained; but that it should have been made at all, and answered as above in 1828, shows that for some time before changes were at work, either in the facts, or in the minds of the observers, corresponding with the statement attributed above to Dr. Alison, that from 1829 onwards the fevers had ceased to be such as would bear blood-letting. In an article published in the same journal in October, 1827,[1] Dr. Alison has very carefully detailed his personal experience of this epidemic, from its origin in 1826; maintaining "that in this epidemic, and particularly during the present year (1827) the danger has appeared much more frequently to depend upon *asthenia,*—on the weakened state of the circulation, than during the last epidemic between 1817 and 1820." In conformity with this observation, Dr. Alison appears in this article to be feeling his way (so to speak) towards that energetic and uncompromising use of stimulants which distinguished his practice at a later date. Not that he had at all lost faith in blood-letting, for all through the memoir he alludes to it as a most important and even essential remedy in some cases, especially when it can be performed early; and this, even in cases characterized by much disorder of the nervous system, or (as he explains in a note) of the "*Fièvre Ataxique*" (p. 253), in which it is all but certain that at a later date he himself would have altogether deprecated depletion at any stage. "In the treatment of this form of fever," he says, "*when seen from the commencement*, I have no doubt that blood-letting is almost always a proper preliminary to all other measures; but after the pain of the head has subsided, although the pulse be of good strength, its beneficial effects are less decided" (p. 254). He even states, in conclusion (p. 259), that "in some which were fatal, I have regretted that more bleeding had not been tried. *But I have been more convinced, during the present epidemic, than I had ever been before*, of the efficacy of stimulating remedies during a great part, at least, of those cases of fever in which, although an evident and dangerous complication of local affections exists, *yet the chief danger appears to depend upon the general depressed state of the circulation.*" The pleading here is evidently in favour of a *new method*, by which stimulants are shown not to be contra-indicated even in the presence of local inflammations; while, on the other hand, depletion as applied to these, and even to the presumed "inflammatory fever" itself, *at the beginning*, is still maintained as essential. At a later date, probably of the same epidemic, the evacuant method gradually gave way in Dr. Alison's convictions, and the stimulating practice reigned alone. And hence he taught in general terms that, after 1829, he had ceased to observe fevers that would bear blood-letting. Dr. Christison, however, certainly, and Dr. Alison, probably, maintained in theory to the last that in *synocha* like that of 1819 blood-letting was still admissible, if not indispensable; only (as the former remarks in his Autobiography, p. 151), "although we have had two great epidemics since, in 1842-43, and 1847-48-49, consisting, as in the earlier epidemics, of both typhus and synocha in its relapsing form, there never has been again that high state of inflammatory reaction of the circulation which formerly made the treatment by blood-letting useful and practicable. Dr. Christison, however, places the date of this "change of type" later than Dr. Alison, viz., in 1833,[1] at which date he "altogether gave up blood-letting in fever."

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[1] "Observations on the epidemic fever now prevalent among the lower orders in Edinburgh." This was probably Dr. Alison's first published contribution to the subject. *Edinburgh Medical and Surgical Journal,* vol. xxviii. p. 232.

It may be added, that the maximum of stimulant recorded by Dr. Alison in the memoir above referred to is eighteen ounces of port wine and a bottle of porter in a man 45 years of age; an amount continued only for one day, and speedily reduced. The only other nourishment specially mentioned is beef-tea. The diet, however, is not at all insisted on; and milk receives no notice at all. Dr. Graves's maxim, to "feed fevers," seems as yet not to have emerged.

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[1] Autobiography: *Life*, vol. i. p. 150. I am at a loss, however, to understand the statement which follows:—"No such change occurred in the second great epidemic of 1827-29." Dr. Alison's strictly contemporary evidence, as above quoted, is exactly to the opposite effect; even if we should disregard the testimony of the anonymous reviewer whom I suppose to be possibly Dr. Christison himself.

IV.—COURSE OF TYPHUS FEVER, AND PHENOMENA OF THE CRISIS.[1]

WE are now in the midst of a very considerable epidemic of typhus fever, and, under the impression that it might be useful to you as well as a relief to the medical staff of the fever-house, I have been on duty in two fever-wards since the 27th of September last, and have seen to an end considerably upwards of 100 cases, of some of which I gave you some account in a previous lecture. My winter engagements, however, oblige me to abandon fever duty at present, and we must consider the programme of clinical instruction in fever as cut short abruptly at this date. We have had, however, a very considerable field of experience, and as I distinctly intended from the first that these cases should be made subservient to your instruction, I am unwilling to depart from them now without some attempt to clear the way to principles which may possibly be of service to you. I have determined, accordingly, to publish in the *Glasgow Medical Journal*[1] an abstract of all our cases, that you may be able to refer to them as the basis of facts upon which this lecture, and another which will follow it, will be founded. You will find in the journal alluded to the notes of 68 cases of febrile disease in the male ward, and 58 in the female ward, numbered *successively*, exactly as they were admitted. Of these 68 male cases, we regarded 58 as genuine typhus, established as such within at least a few days after admission, and beyond all reasonable doubt; and of the 58 female cases we regarded 50 as genuine typhus, the remainder being cases, one or two of which *may* have been typhus, but which, on the whole, we regarded as either doubtful, or decidedly *not* typhus fever. Every case noted as typhus was so noted on grounds which you may read in the abstract to which I have referred, and which, I trust, will bear the strictest investigation.

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[1] Clinical observations delivered at the Royal Infirmary of Glasgow, December 15th, 1864.—*Lancet*, 1865, vol. i. 56.

Now this disease—typhus fever as witnessed by us—is one of which the following brief summary may be given. It is a disease very decidedly and strongly febrile, in which the pulse has rarely been less frequent than 100, often as frequent as 120, not seldom 130, or even 140 or more, in the minute; but in this last case usually, in the adult at least, indicative of grave danger. Moreover, typhus fever as we have seen it of late, is a disease almost always indicated by a peculiar rash (which has been so often described that I will not linger on the description now)—a rash appearing during the first week, and persisting during a large part of the second week, and then gradually dying away. Again, the typhus with which we have had to do has been marked by very decided, and not at all equivocal, nervous symptoms of the kind called typhoid, of which the first and least characteristic was sleeplessness, extending over several nights; the next delirium, often with tremors and agitation, muttering, and more or less contracted pupils; the last and worst, coma, with subsultus tendinum, and failing of the vital powers, pulse, and animal heat, from which last and worst state few escaped with their lives; but happily coma was present only in a small minority of the cases. We have also had a considerable number of complications, of which I will only name the following at present:—Bronchitis, frequent and severe in the latter part of our two months' experience; diarrhœa, frequent, but not very severe; vomiting; epistaxis; menorrhagia; suppuration of the ears, with nervous symptoms apparently depending thereon; abortion; jaundice; laryngeal complication; erysipelas; paralysis of portio dura; inflammation of the femoral vein; albuminuria. A considerable list, assuredly; but then several of these complications were observed only once. On the other hand, several of the deaths were manifestly due to the complications, as you will see further on.

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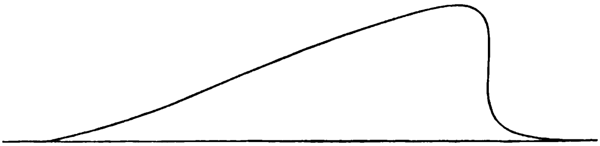
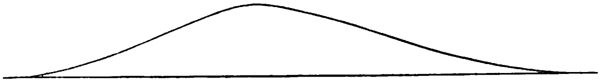
[1] "Two Months of Fever Duty in the Glasgow Royal Infirmary." January, 1865.

The next point I wish to notice in regard to the course of typhus fever as observed in these cases (and it is one of such importance in a practical point of view that I shall have little else to bring before you to-day), concerns the *phenomena of the crisis* (so-called), especially in the cases which have ended favourably. As a great number of patients affected with typhus fever get well, while a certain proportion die, I need hardly point out the immense importance of noting the circumstances which determine one or other of these results, and particularly the symptoms which, in particular cases, give you the earliest assurance that the disease is going to abate. To this inquiry we have given particular attention, and the result is, in the first place, that the well-known term *crisis,* in the old and figurative but still strictly true application of it to some febrile diseases, is scarcely a description of what occurs in the fever with which we have had to do. Let me recall to your remembrance the meaning of that word. Etymologically it presents to the mind a mysterious supernatural authority or law—a Nemesis (let us say)—passing *judgment* on the issue of the disease, with power to make or mar, and that instantly. At a certain hour the judgment goes forth: "This disease shall depart, and the patient shall live." And forthwith the sentence is obeyed, the disease vanishes. That is, or was, the idea of a crisis, and it was further observed to be accompanied in many cases by certain evacuations, which were called on this account *critical*, and supposed to be, in fact, the means by which the disease, or the material cause of the disease (*materies morbi*), was carried off or expelled from the body. I am not concerned at present either to attack or to defend this old pathology; but there can be no doubt that this idea of a crisis harmonized well with the facts of many fevers as observed by those ancient physicians. But in typhus fever, as we see it at present, there is absolutely no trace of a crisis of this rapid and decisive kind. The fever is not, apparently, judged, or condemned, and forced to vanish suddenly, after the manner of an evil spirit fleeing from the sentence of an invisible power. Nor does the resolution of the fever almost ever take place in company with marked evacuations, such as could possibly have suggested the idea of a purification of the system from a "matter of disease." On the contrary, the whole process is devoid of startling phenomena; it is gradual, distributed over days, and often for days imperceptible to all but the most minutely accurate observation; there is neither sweat, nor diarrhœa, nor any striking change in the urine; and if any of these should by chance occur, they are just as likely as not to be non-critical and of unfavourable import. This is no new doctrine as regards typhus fever, but it requires to be brought into stronger relief than in most of the current authorities; and the facts of this particular epidemic, aided by the more general recollections of a good many years past, enable me to do this with perfect confidence as to the accuracy of the picture which I shall draw for your instruction, not from books, but from direct observation. For although the disease with which we have had to do has had no *crisis* in the ancient sense of the word, it has nevertheless had a very regular and *normal course of increase and decline,* to which I now invite your best attention. The character of the disease has been, that it rises, often with perfect regularity, though quite gradually or even imperceptibly, to what may be called an acmé; and then, with no less regularity, and not less gradually, declines towards the state of complete convalescence, often without any evacuations or sudden startling phenomena of any kind throughout. The rise and fall of the fever are equally gradual; and the curve indicating the fever—if one might represent it roughly (but still truly) in the form of a diagram—would not be after the ancient idea of crisis, thus:—



but rather thus:—



an acmé, or single summit, or maximum point of development, being characteristic of both the diagrams; but the one which represents typhus differing from the ideal one of a crisis in the perfectly gradual character of the decline. From this normal type I hold that typhus fever very rarely departs, unless disturbed either by complications or by injudicious treatment; and therefore it is literally of immense importance to your comfort and security in treating the fever, as well as to the patient's security, which is of even more importance than yours, that you should know accurately the rules by which you are to watch for this slow and gradual crisis.

Now, what are you to look to with the view of catching the first glimpses, the first favourable indications, of this important movement? I believe that, speaking practically, and speaking of the great mass of cases, it is *the pulse;* and especially, I would say, the *morning* pulse, which is the least apt to be disturbed in its indications by accidental causes. The internal heat, as determined by the thermometer, is, I believe, a very valuable indication;[1] but the thermometer is so much more cumbrous as a means of investigation, so much more difficult to apply, and takes up so much more time in the case of a busy practitioner having many cases to observe, that I believe the oldest of all studies in fever, that of the pulse, will hold its ground practically, at the bedside and in ordinary investigations, against every other. It is easy, however, to study pulses in fever, without arriving at an idea of how valuable and delicate a test of the crisis we have in the pulse, when watched for days together, and in connection with an estimate, as accurately made as possible, of the duration of the disease. Of this I am sure, because I have again and again had my own observations criticised and doubted by good observers, and have had to come back to nature again and again to establish to my own satisfaction the points to which I have now to direct your attention, as regards the pulse-rate in typhus. What I have now to tell you has been carefully tested by the facts of the present epidemic, but is also consistent with facts observed by me on various occasions, at least since the year 1857, and particularly referred to in a publication of the year 1859, on the history of epidemic fever in Edinburgh.[1]

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[1] See further observations on this subject at p. 191, *infra.*

There is one stipulation I must make with those who desire to follow out this inquiry with the view of testing the normal mode of the crisis in typhus fever. It is, that *as many cases as possible should be left to their natural course, unaffected by either drugs or stimulants.* Of course, I should never think of recommending to you this mode of proceeding, unless I believed firmly that the disease could be so treated in many cases with advantage to the patient, as well as with increase of knowledge to the physician. But this is precisely what I do believe most firmly, and what I mean accordingly to occupy another lecture in impressing upon you by arguments drawn from experience. Meantime let me state, as matter of opinion, that not only am I convinced of the safety and expediency of leaving many cases of typhus to take their *normal* course, but I further believe the normal course may be very easily altered for the worse by what is called treatment; and in particular, as regards the period of the crisis, I believe that the habitual or constant exhibition of drugs and stimulants has a great tendency to mask the disease, to disturb or to retard the crisis, and by so doing to increase the mortality. This is an opinion formed after a most careful observation of particular cases in detail over many years, and I advance it now instead of waiting till next lecture for its fuller development, chiefly in order to add, that you cannot possibly observe the natural course of typhus unless you can make up your mind beforehand to leave many cases to follow their natural course. I am fully convinced that there are many practitioners who scarcely ever see a normal case of typhus, owing to their perpetual and systematic interference by drugs and stimulants; and it is even a question with me if the written descriptions of previous epidemics have not been largely vitiated by this cause, the disease being to some extent, as it were, disguised, or perverted from its natural and favourable course, by the treatment. How far this may have been so in fact I have no means of knowing exactly; but I have had abundant opportunities since the year 1857 of ascertaining that typhus fever, both in Edinburgh and Glasgow, has followed the normal course now about to be briefly indicated.

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[1] *Edinburgh Medical Journal*, July, 1859; and *Clinical Medicine*, 1862, p. 164.

There are, however, one or two further precautions that must be taken before you can hope, I do not say to avoid disturbing the crisis in typhus fever, but to avoid killing your patients outright, or rather, letting them die of sheer neglect. You must feed your patients, and you must feed them chiefly on milk. Milk or buttermilk is with me the staple food in typhus; and I will even say that I know no other food that can be depended on. Yet I see, and always see with a new surprise, descriptions of the treatment and dietetics of fever, in which not a word is said about milk, and a great deal about beef-tea, wine, whisky, brandy, and all manner of things supposed to be more strengthening, or stimulating, than milk-diet. Now, I tell you frankly that treating fever patients without plenty of milk is a thing that I do not understand at all; for I suppose I have not treated a single case of fever of any kind for the last fifteen years (I cannot make precise statements beyond that date) without milk, and I always proceed on the understanding that milk in fever is the one thing needful as diet—always to be given, and given liberally, whether specially ordered or not. To give wine, whisky, and beef-tea, while withholding milk, is simply, in my opinion, to destroy your patient; and the more wine or whisky you give, while withholding milk, the more sure you will be to destroy your patient soon, because you are thereby superseding the natural appetite (or what remains of it) for a nourishing and wholesome diet, by a diet—if it can be so called—which poisons the blood and checks the secretions, and alters for the worse the whole tone of the nervous system and of the digestion and assimilation. I believe that infinite mischief has been done in typhus fever, and in all fevers, by giving wine, and withholding or not giving milk. Under a false theory of administering alcoholic food, it has resulted, not only that natural and genuine food has been withheld, but that the small remaining amount of appetite for such food has been obliterated, and not unfrequently, even at an early stage of the disease, the patient has been practically disabled from taking any proper nourishment at all. I know, unhappily, as a fact, that not only doctors, but nurses and patients, and patients' friends, are readily brought under the influence of this fatal delusion, that alcoholic liquors can, in fevers, take the place of natural food. But it is none the less a fatal delusion, and I warn you solemnly against it; all the more that the patient is absolutely in your hands, and you can obtain no guidance from his natural instincts, if you begin by overwhelming them with large doses of alcoholic stimulants. I have been very careful, at least for fifteen years past, to avoid this error, and I believe that any success I may have had in managing fever has been more due to this than to any other cause. But I allude to the matter now simply to say that if you would observe the natural or normal course of typhus fever, with respect to the crisis (as proposed at present), you must absolutely make up your mind to feed your patient naturally, and not to over-stimulate him. An opposite course leads to consequences which I may consider more in detail on another occasion; but what I am anxious to say now is, that it tends to disturb the course of the fever, and to retard the crisis.

Further, you must, if you would observe the natural course of typhus fever, give your patient plenty of fresh air. This is now so very general an admission that I need hardly have alluded to it, were it not that adequate ventilation is still apt to be forgotten in treating fevers in the houses of the poor or in badly-constructed hospitals. With adequate ventilation, you must of course, especially in winter, have the means of warmth; and great attention must be paid to cleanliness, so far as this is possible in bedridden and exhausted patients.

These, I think, are all the precautions necessary, I do not say for the proper treatment of typhus fever, but for the proper observation of its natural course in normal cases.

Now I venture to assert—and I make this assertion not by way of theory or opinion, much less prediction, but as a statement of facts, founded on a rather extensive experience ever since 1855 or 1857—I venture to put it forward as a law, or generalization, with respect to typhus fever:—*That in a large proportion of cases, typhus fever, left to its natural course, and treated with abundant milk-diet and without drugs or stimulants, will have its natural crisis before the twelfth day.* Of course you will understand that I use the word *crisis* in a modified sense; for I have just been explaining that there very rarely is in typhus a true crisis, in the ancient sense of the word. What I mean is, that most commonly before the twelfth day, very frequently as early as the ninth or tenth day, sometimes, though rather rarely, even before the ninth day, indications are to be perceived that the disease is about to terminate favourably; and that these indications are to be trusted, even when some of the most formidable-looking symptoms continue present to a later period. And this I confidently assert to be a very general law, or *norm* (to use a word borrowed from the Latin, but not yet naturalized in English, except in the adjective form *normal*), of typhus fever as observed in Edinburgh and Glasgow of late years. Observe, I do not venture to assert that it has always been so, or will always be so; on the contrary, I have myself remarked the tendency to a delayed crisis as an epidemic fact at certain seasons; but in a large proportion of cases, and excluding certain exceptional groups of cases, it has been so, in the main, in every year from 1857 to 1862 in Edinburgh, and during a large portion of the epidemic of the last two years in Glasgow.

If you wish to have the satisfaction of getting the earliest information of this early crisis in typhus, *watch carefully and constantly, from day to day, or several times a day if need be, the rate of the pulse.* For this is, speaking practically, the guiding clinical fact in the natural history of typhus fever. Making allowance for certain obvious, and a few not so obvious, causes of accidental disturbance, you will find that the pulse pursues a very regular course of rising frequency up to the acmé, and declining frequency beyond the acmé, of the fever; and that although the other symptoms very surely follow the pulse in uncomplicated and regular cases, yet they often follow at some distance behind: so that the crisis can be ascertained for practical purposes, and particularly for the great purpose of guiding treatment, more accurately and effectually by frequent noting of the pulse-rate than by any other method. I have ample proof of this in the cases of the last two months, for which I will refer you to the hospital case-books, and to the abstract already referred to.[1]

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[1] See especially the cases enumerated, *loc. cit* above (p. 159, *note*), which comprise a list, not of the whole, or nearly the whole, but only of those which could be pretty clearly and definitely ascertained to

This accurate and frequent noting of the pulse-rate in typhus, in connection with the day of evolution of the disease, is of immense importance as regards the prognosis; for not only are you able to predict the recovery with greater confidence, and at an earlier period than you could do otherwise, but you have often the first intimation of complications, either during the fever or during the convalescence, through some unaccountable departure from the *norm*, or regular gradual increment and decrement of the pulse. In fatal cases, where the death is from *pure* typhus, the pulse usually becomes more and more rapid, and at the same time weaker and smaller, up to the very hour of death. In

have had a crisis before the twelfth day. It is needless, and would be very fallacious, considering the difficulties that are well known to observers in fixing the date of the fever in many instances, to attempt an exact numerical estimate; but among the males, 33 out of the whole 58 cases of typhus fever could be clearly indicated as having this early crisis, and in no one of these did any retrograde movement occur as regards the pulse, though in many of them the other symptoms of improvement were long delayed. Among the females the proportion of early crises was smaller, and perhaps also the individual observations were somewhat less minutely detailed in some of the cases, so that only 18 cases of early crisis are clearly definable among 51 cases of typhus fever. Read, in particular (*Males*), Cases 6, 11, 28, 30 (day not quite certain, but early, though delirium protracted), 61 (dangerous bronchitis, but still early crisis); (*Females*) Cases 17, 20, 22, 33. Several of these cases were read and analysed in detail in this lecture as delivered, but I have deemed it expedient, in revising for the press, to omit the details of cases, and alter considerably, in accordance with this omission, some of the preceding paragraphs.

complicated cases, where the complication is not very dangerous, the pulse may be merely quickened for a day or two, or the crisis may be protracted, or the pulse may begin to rise again, after a quite distinct critical subsidence. In very dangerous complications the pulse is out of all bounds, and no rule can be assigned for its irregularities. But in regular and normal cases, being uncomplicated—*i.e.*, not having any positive organic or inflammatory complication,—the decline of the pulse, even by a few beats, before, at, or about the twelfth day, carries as an inference the probable decline of the fever; and, on the other hand, a delay of the crisis very much beyond this is to be regarded with suspicion, though not by any means necessarily of fatal tendency.

But the most curious inference that I have been able to draw from the cases of typhus which we have been observing for the last two months is this:—If, in a particular case, you observe that the pulse keeps steadily to the *norm* or law of uncomplicated typhus, as I have endeavoured to exhibit it; if the rate of the pulse increases steadily and gradually up to the ninth, tenth, eleventh, or twelfth day, and then, at any period between the eleventh and fourteenth day, is observed to be undergoing a correspondingly regular decrease, not rapid, but regular and gradual; *in such a case of regular crisis indicated by the pulse-rate, it is almost insignificant, as regards the prognosis, that the other symp-*

toms continue formidable-looking for days; for in all such cases that we have observed in hospital lately the patient has got well in the end, and that without further treatment. Thus I can tell you from experience that, after a regular crisis of this kind, the tongue may be as black as pitch, and so dry that it feels like a bit of roasted leather, delirium may go on, the patient may be out of his bed many times in a night, the skin may be hot and dry, the eruption brilliant or livid for days together; still, if you have the favourable state of the pulse, and if there is no complication, *the patient will get well.* The pulse is your criterion. Whenever it is fairly turned, and declining in rate, especially if it is at the same time gaining in strength and volume, you may be at your ease, unless there is reason to suspect a complication; and even then you may be at your ease, if the complication is not severe. But observe, *only if you do not interfere.* The proper course in such a case is to take confidence from the pulse, and avoid interference. *You can afford to wait.* And only one more word to-day, but it is a very startling one, and yet true according to my experience: even if the delirium should become worse after the crisis as indicated by the pulse; nay, more, even if the patient has had little or no delirium before, and *begins* to be delirious for the first time after the pulse has begun to show symptoms of crisis; still, it is better to wait, for in all probability he will be quite well in a few days, provided you do not disturb the course of the fever

Continuation of preceding lecture, December 22nd, 1864.[1] I endeavoured in the previous lecture to establish the fact that typhus fever is a disease which, within certain limits of variation, may be observed to run a regular or *normal* course, and that the treatment of the disease throughout should be guided by a close and exact observation of its *norm* or law of progress, and particularly by the facts connected with the rate of the pulse, as I detailed them to you. I now advance the position (which, I think, is not more than will be readily allowed by the majority of well-informed practitioners), that for the *fever* in typhus, considered merely as a fever, we have as yet no specific remedy—nothing corresponding to the undoubted action of quinine in ague, or to the supposed, but still controverted, action of mercury in syphilis. Given a case of typhus fever, merely as such, and without knowing any more about it than that it is a case of typhus fever, there is literally nothing in the way of medicine that you can seriously propose with the view of modifying considerably the normal course of the fever, or at least of modifying it favourably. You literally cannot control the disease, in its regular normal course, by medicines at all. You may very possibly make it worse. You cannot, according to the present state of our knowledge, do anything at all to make it better, upon the principle of an antidote or specific for the fever as such.

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[1] Revised from short-hand notes made at the time, but not previously published.

I am endeavouring, you will observe, to state to you calmly and impartially—to define to you with scientific exactness, as it were—the present position of our knowledge. It is upon this point, I admit, not quite satisfactory; but the next best thing to having knowledge of a remedy is to be willing to know, and to admit, the fact that we have none. I do not say that you are to rest in this position. I have tried, or seen tried, nearly all the favourite remedies in typhus fever, at different times, and no one will hail with more pleasure than I shall the advent of any true specific for typhus; any remedy truly *antidotal,* of which it shall be proved incontestably, or even presumed upon fair grounds of evidence, that it has the power of controlling and cutting short the disease. But after multiplied trials, and ages of controversy and discussion about typhus, the present state of the question is that no such specific has been discovered, and this it is my duty to tell you, just as much as it would be my duty to tell you of an antidote if it existed. There is nothing to be gained in the end by mystification upon a point like this, and I choose to tell you exactly, therefore, so far as I can, what is known and what is not known as to the power of remedies to control typhus fever.

Well, then, if it be true in general that you cannot control, and especially cannot shorten, typhus fever, and if it be true (as I endeavoured to show you in a previous lecture) that you can usually ascertain the *normal* *crisis* of the disease as early as the 9th, 10th, 11th, or 12th day, I think you will admit that it is plainly your duty to watch for this by daily, sometimes by hourly observations, conducted on a carefully-regulated plan, so as to be able to guide your treatment in detail, and assure yourself as to whether the individual case is keeping within the *norm*, or if not, what is the cause of the disturbance in the normal course. And remember, I give you for your comfort this instruction, that, generally speaking, when you have ascertained the fact of the crisis (chiefly through the regular and determinate fall of the pulse from the 10th or 12th day onwards), you may practically regard your patient as almost safe,[1] and this even though the typhoid delirium should not immediately give way, or even if it should threaten to increase a little after the crisis. But you will not have the slightest security for the safety of your patient, under these circumstances, unless you have ascertained by frequent and carefully regulated observations all the facts on which, in general, you have to rely for your knowledge of the crisis, and of the states which may indicate danger.

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[1] There will be apparent exceptions to this rule, as to all general rules, and the exceptions will be very difficult to refer to a principle. Some time ago (1869) I saw, with Dr. Gray, a lady with many signs of crisis, but still with contracted pupils, and stupor deepening into fatal coma late in the disease. One of the medical assistants in the hospital, seen by me throughout with Dr. Andrew Buchanan, had an apparent crisis about the 15th or 16th day, with a receding eruption and a lowering pulse, followed by a very dangerous aggravation of cerebral symptoms, from which complication he barely escaped with his life about the 21st day. More lately, a Catholic priest, seen with Dr. Scanlan, with an apparently cleaning tongue and somewhat lowering pulse, and very free from typhoid delirium, but greatly depressed in spirits about himself, perished rather suddenly about the 17th day of the disease, by pure asthenia. And in the sad case of a professional brother, seen once only with Drs. Marshall and Wallace of Greenock, a falling pulse for two days, and a cleaning tongue, with increase of general sensibility, but with

Now all this has a most important bearing on treatment, because, even admitting that no active routine treatment is to be pursued in typhus, it is just at these later critical stages that the hand of the practitioner, or rather the mind of the practitioner, is most apt to be unsteady. And I know from experience that it is at times an awful responsibility, a most serious burden for the mind, the feeling of dread that you may do something wrong, or omit something that ought to be done, at the critical period of typhus. I know nothing, indeed, in medical practice more serious. And although my own practice, as you will observe, tends on the whole to what might be called the inert, or to *expect-*

rather contracted pupils, were followed by convulsions and a rapidly fatal termination about the 12th day. I have remarked upon one or two similarly exceptional cases (*loc. cit. Glasgow Medical Journal*, Case 47, *Males*; Case 31, *Females*); but according to my experience, they are both more frequent and more fatal among the better classes, and especially the professional classes, than among hospital patients, in whom, probably, typhus fever is altogether a less formidable disease than in the classes who live by the labour of the brain.

ancy (if you like a grand name for it), it does not tend to this from any want of carefulness in watching symptoms, and certainly will not be correctly imitated by you if you fall into habits of carelessness as regards the watching of individual cases. On the contrary, you must base your *expectancy* (when you think it right to *wait*) upon a minute personal knowledge of the *norm*, or law of development, of the fever, and on careful daily, sometimes almost hourly, attention to the evolution of the fever in each individual case. And when I say, as I now say in general terms, that *whenever you can see your way to it, you will do well to leave the patient in typhus fever without either drugs or stimulants,* I do not mean that this, any more than the opposite plan, is to be adopted as a blind routine, exempting you from the careful observation of details, but quite the contrary. My principle, in other words, is this—that the blind or aimless administration of drugs and stimulants is not only unnecessary, but positively bad practice; because drugs and stimulants, especially in heroic doses, may very well make the fever worse, and in ordinary or normal cases cannot possibly, so far as we know at present, do anything to make it better. They may poison the blood, and disturb the course of the disease, and lead to fatal complications, but cannot possibly, so far as we know, do anything to control or annul the fever-poison. And, on the ground of theory and experience taken together, I think it a far more satisfactory, as well as a more successful practice, to watch well the course of the disease for accidents specially requiring interference, than to be making an artificial disease out of the natural one by constantly disturbing the course of the latter, when it is known to be passing, in the great majority of cases, towards a favourable termination. But then, observe, that instead of demitting you from the duty of watchfulness, this practice increases the necessity for watchfulness on your part. For I can say with perfect truth, speaking for myself, that I should never have *dared* to withhold what, in my student-days, was taught as the ordinary routine practice in typhus—nay, I should not dare, even now, to stand by, and watch, and do nothing, except from a feeling, based upon experience and strong personal conviction, that the law of typhus is what I have endeavoured to represent it, and that each individual case in which I think it right to *wait* is keeping within the law, and not diverging from it in any particular direction requiring special interference.

What, then, are the principal accidents that you have to be looking out for at every hour and every turn of the disease? It would take a long time to discuss them in detail, in connection with the appropriate treatment of each; but I will merely enumerate a few, in order that you may fully appreciate the necessity for watchfulness.

1. You have to be constantly on the watch for

pulmonary and bronchial complications, which are often severe, and sometimes very fatal in typhus fever. Several of our cases were in great danger from this cause. I do not mean, however, that you are to be constantly auscultating the chest, for this will do more harm than good on the whole; but you are to observe closely the breathing, the pulse, the presence or absence of cough, expectoration, dyspnœa, lividity, etc., and to accustom your nurses also to observe these symptoms; for over these, especially in the early stages, you have considerable control by means of remedies; especially antimonials cautiously given, and also ipecacuan, squill, camphor, and other expectorants. And remedies here may often make the difference between life and death.

2. You have to be on the watch for diarrhœa. I do not know, indeed, that this is a very fatal symptom in typhus, or that you ought to exercise to much control over it. But although it may sometimes be allowed to take its course, it may also occur under circumstances requiring control, and you must inquire and know about it daily when it does occur.

3. You have to be attentive to all the little details which tend to procure sleep at night in the early stages; because any inordinate sleeplessness at this period is more likely than anything else to lead to exhaustion and typhoid delirium in the later stages; and after two or three nights' tossing in the early stages of the fever, I think an opiate or some other form

of narcotic, or other sedative of the nervous system,[1] is often very legitimate practice. But observe, *only* in the early stages. When typhoid delirium has fairly begun, and especially after the 9th or 10th day, sleeplessness at night is then strictly a normal feature of the disease, and you cannot procure good sleep by opium, though you may very easily poison the blood, and bring on coma, and even convulsions. To make opium safe in typhus, you must have a moist tongue, abundant excretions, and an absence of delirium. Under these circumstances, a *single* opiate, given so as to imitate natural sleep, will sometimes be successful; but sometimes also it will fail, and I am far from thinking that it should be often repeated.

4. You have to exercise a constant care to have your patient properly fed throughout, otherwise he will very probably sink from sheer starvation. This, too, like everything else in fever, requires an attentive nurse, and you can only make your nurse attentive, in many cases, by constant inquiries based upon your knowledge of the disease. You should, therefore, not

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[1] Possibly, bromide of potassium may be found to accomplish this indication in some cases; but my experiments with it in typhus fever hitherto (1869) have not been so satisfactory as in some other forms of disease attended by sleeplessness. Dr. Russell has pointed out, and I agree with him, that this object of inducing sleep may sometimes be safely and well accomplished by a *single* considerable dose of one of the stronger alcoholic stimulants, given so as to have the effect of a quieting draught, in states of prolonged nervous irritation

only (as Dr. Graves said) "feed fevers," but you ought to see that the instruction is carried out, and how it is carried out. On the principles of the feeding to be employed I have already spoken in last lecture.

5. You will have to be constantly on the watch for retention or suppression of urine in the course of the second week, sometimes in order to give directions, sometimes in order to interfere by the use of the catheter several times a day.

6. You will also require to be on the watch against the formation of bed-sores, and generally to see that the skin of the patient is kept as clean as possible, his position in bed shifted from time to time, the bedding and linen frequently changed, and all his natural wants carefully attended to. It is the characteristic of a good nurse to attend to these things, and of a bad one to neglect them; and generally it may be said that in fever wards, unless the physician makes many inquiries, and is very watchful and exacting as regards such details as these, the nursing is sure to degenerate.

7. Delirious patients of course require special watching, and great firmness and self-possession on the part of the nurses, to prevent accidents.

8. You have to be on the watch against sinking of the patient from pure exhaustion, or from sudden collapse, especially from the 10th day onwards; and it is here that the question of the use of stimulants presents

itself as a strictly practical question, not of routine, but of refined experimental therapeutics. The theory of alcoholic stimulants is a very wide and difficult subject, which I cannot undertake to discuss at present; perhaps, indeed, I should not be able to give you, under any circumstances, a perfectly clear view of it. But I mean to devote the rest of this lecture to a statement of some of the views by which I have been guided, and some of the results at which I have arrived, in treating cases of fever in this hospital and elsewhere.

Note as to Various Opinions on the Alimentation of Patients in Febrile and in some Chronic Diseases; and especially upon the Uses and Abuses of a Milk Diet. (*September*, 1888).—There are some very notable differences between the first edition, published in 1862, and the second (1873) of Dr. Murchison's classical *Treatise on the Continued Fevers of Great Britain.* A few of these may be here noticed as tending to show both the abuses current in London at the former date, and the gradual reaction towards a more reasonable and normal state of opinion, as it appeared to a very unprejudiced mind conversant with London practice. Dr. Murchison is probably entitled to the credit which he claims in a more recent contribution[1] of "having been one of the first to raise my voice against the fashion, prevalent about twenty years back, of treating all acute diseases, on principle, with large doses of alcohol." He, in fact, argues the question upon lines not very dissimilar from those of the preceding articles, the main argument of which was set forth by me, quite independently, in 1861, in a paper above referred to, in the *Edinburgh Medical Journal.*[1] But in the alimentation of typhus fever, as described in detail in p. 254 of the first edition of Dr. Murchison's book, milk is but little insisted on; whereas in the second edition he adds to the enumeration (more generally given than in the first) of the other forms of liquid food, the following confirmation of what is here maintained:—"Of all these forms of nourishment, I agree with Dr. W. T. Gairdner in thinking that milk is the best. I have for many years been in the habit of giving it in preference to beef-tea" (p. 286). Dr. Murchison had in the first edition contrasted the starvation treatment in fevers (so strongly condemned by Dr. Graves) with that followed in London at a later period. "There is reason to fear," he writes, "that the tendency of modern practice in England is not to starve fevers but to overfeed them. I am persuaded that incalculable injury is sometimes inflicted by forcing food down the patient's throat every half-hour, or every quarter of an hour. The patient is not allowed a moment's peace, while the food is not absorbed, but *is discharged little changed from the bowels, and often induces diarrhœa and tympanites.*" The implication is that these criticisms are intended to apply to the practice (as then actually observed by Dr. Murchison) of Dr. Todd and his followers; or, at all events, of the many who, under the influence of his teaching, but possibly even exaggerating his methods, gave practically unlimited doses of brandy and beef-tea constantly by day and by night, after the fashion condemned in the preceding articles. And it is, perhaps, noteworthy that the latter clauses (indicated above by italics) of Dr. Murchison's criticism are suppressed in the edition of 1873, when the practices alluded to, and the evils directly springing from them, had probably become considerably modified.

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[1] *The Alcohol Question:* by Sir James Paget, Bart, etc., etc. Strahan & Co. (no date; but actually published about 1880).

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[1] *Clinical Medicine*, pp. 64-68; compare also pp. 74-78 (first published in 1858).

Like everything else in fever, even milk, of course, may be given in excess; in which case it is very easy to admit, with Sir William Jenner, that "the indiscriminate employment of milk in almost unlimited quantities as diet in fever has led to serious troubles." All that need be said here in reference to the particular evils recorded in the admirable paper here referred to[1] is that they do not occur, or occur very rarely indeed, in our large and well-appointed Glasgow Fever Hospital, where milk is very extensively, indeed, all but universally employed, but at the same time is employed under proper control. The principal caution required to avoid all such evils is that pure cold water shall be at least as accessible, and as frequently presented, as milk, or shall be employed so as to dilute the milk considerably, in case of excessive thirst. It is quite true that good milk, undiluted, *if taken largely as a drink and not as a food*, tends to excess; for, as Sir William Jenner well remarks, "a pint of milk contains as much solid animal matter as a full-sized mutton chop." But the state of medical opinion with which I had to deal in this and the preceding article was one not at all leading up to an excess in the use of milk, which was, indeed, scarcely employed at all by the devotees of brandy and beef-tea. With Sir William Jenner's suggestions and restrictions on the subject of alcoholic remedies in typhoid fever, I am entirely in accord.

When engaged in lecturing on the fevers in 1882, I desired to have the means of bringing under the notice of my students, more formally than before, the practice at that time followed in the City of Glasgow Fever Hospital in respect of the dietetics of fevers. I therefore applied to Dr. Allan, the excellent and experienced medical superintendent, who, with his numerous and qualified assistants, may be said for many years past to have had almost the entire control of the epidemic field of Glasgow and its neighbourhood, so far as the treatment of febrile diseases is concerned. I specially requested, not only that my own views and teaching as to milk diet in fevers should be considered as open to criticism, but also that all the objections which have taken shape in any way as to milk, and all questions of danger or inconvenience arising from its use, or even abuse, should be carefully considered. The result was a letter, dated 20th January, 1882, the substance of which appeared in *The Lancet* for August 18th, 1883. This paper will be found of great interest, and of permanent value, as a record of impressions carried down to a date twenty years later than my contributions to *The Lancet*, in an hospital wholly withdrawn from my personal intervention and control, though not, perhaps, uninfluenced by these and other instructions. Dr. Allan's opinion, in general terms, is that the objections to the use of milk as a principal, though not exclusive, diet in fevers (and especially in enteric fever) " either disappear, or are reduced to very small bulk, when closely investigated;" that most of these objections spring from the injudicious use of milk in excess, and undiluted, in the case of patients who are thirsty. Dr. Allan believes that no hard and fast rule can be established, either as to the degree of dilution, or as to the total amount of milk to be given; but he inclines " to put the *maximum* allowance of pure milk at (say) four pints"; and to regulate the allowance, short of this, by the observed facts of each case. " When the patient's stomach is very irritable and vomiting persists, we add lime water to the milk in varying proportions—from a tablespoonful to the mug, up to 'half-and-half.'" Soda water, and ice, in the milk are also useful in certain cases; as are watery drinks in general, iced; but not lemonade; because Dr. Allan's experience has led him to think that this, taken as a beverage, tends too much to displace milk, and also to cause flatulence. On the whole, therefore, though not opposed to certain other forms of liquid diet in certain fevers, Dr. Allan holds to milk, more or less diluted according to circumstances, as certainly the staple, or typical, diet, in all febrile diseases, and especially in enteric fever; and he affirms "that if, by accident, our milk 'turns,' or we are disappointed in our supply, we are at our wit's end; for there is nothing that can take the place of the precious milk. And milk is not only a safe and suitable food for enteric cases; when diluted and iced it is also a grateful drink; and it forms as well a valuable vehicle for stimulants, and even for medicines."

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[1] *The Lancet*, Nov. 15th, 1879, p. 716.

The reader may be referred to the entire communication of Dr. Allan in *The Lancet* (see above) as a corrective, founded on clinical experience, to prejudices still entertained, perhaps, in some quarters even in this country, and certainly strongly held in Germany, where, mainly on theoretical grounds, milk seems to be still very sparingly employed, or, at least, has not been advanced to the position assigned to it in our Glasgow experience.[1]

I have had occasion, in a recently published volume,[2] to insist, though briefly, on the advantages of a purely, or almost exclusively, milk diet in some of the more chronic forms of disease of the digestive organs in children, in connection with the Continental practice of prescribing raw meat in such cases. The unreasoning, or at least unreasonable, distrust of a milk diet among many persons, especially among women of the higher classes (founded on personal idiosyncracy or acquired vicious habits of feeding) is often such as to give an entirely wrong bias to the dietetics of the sick room, even in early infancy and childhood; and for many years I have been accustomed to point out, in my lectures, that the peculiar fastidiousness, which causes so many cultured persons to recoil from the use of milk when it is taken only as a part of a very complex and highly artificial diet in health, is often entirely overcome in severe organic disease, where milk, and milk often alone, will effectively nourish a person who, when in health, has never, apparently, been able to take it, even as part of a meal, without suffering. I should not have thought, perhaps, of alluding to this subject at present, were it not that even since the first of these sheets was sent to the press, there has appeared a most valuable and striking testimony to the value of an exclusively milk diet in one of the most obstinate and otherwise incurable forms of chronic disease arising from climatic or malarial antecedents in tropical climates. Sir Joseph Fayrer,[1] writing on Chronic Tropical Diarrhœa alba, or white flux, as described by Annesley, Twining, and others, gives the weight of his great authority to the following statements as regards the only treatment by which he has found it even possible to hope for success :—

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[1] See Liebermeister's elaborate dissertation on Antipyretic Methods of Treatment in Ziemssen's *Handbook of General Therapeutics*. English translation, vol. ii. pp. 109, 114.

[2] *Lectures to Practitioners* (jointly with Dr. Joseph Coats). Tabes Mesenterica, p. 67. Longmans, 1888.

"All irritating food must be avoided. *Milk alone* (cow's milk) which, in some rare cases, may be diluted with a third or a fourth of plain water, or lime water, *should be the only food*, and it must be given in small quantities often repeated, say from four to six ounces every hour. By the time the patient is taking two to three quarts of milk in the twenty-four hours, he will find that he obtains from it all the nourishment not only necessary to support his strength, but to enable him to recover from the disease. I commenced this mode of treatment before I returned to England in 1872, and after prolonged experience I find that milk alone is most effective. It needs no addition, and seldom fails to do the good which is expected from it. At first the patient may lose weight, but soon regains and increases it. In ordinary cases it is necessary to adhere to the milk diet from three weeks to a month or six weeks, after which period a more varied diet may be given, though any change or addition must be made gradually and tentatively, with great care. Beef-tea, raw beef juice, or other animal broth, finely minced meat if not previously cooked, a raw egg beaten up with milk, may be tried, but in the early stages rarely or never agree. Before I adopted the milk, raw beef juice was frequently tried, but it always failed. Farinaceous food is equally uncertain, and experience proves that milk *alone* is the best food. If carefully and judiciously given it not only replaces all other food, but dispenses with the necessity for drugs. Tea and coffee, as a general rule, disagree, and should be avoided. Stimulants, for those who have long been accustomed to them, may be cautiously allowed in limited quantities. The best are brandy or whisky with the milk, or diluted with some alkaline water, such as Vichy or Vals. As a general rule, all wines are unsuitable."

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[1] *Ed. Med. Journal*, September, 1888, p. 209.

In reading the works of the older medical authors, one is often impressed with [illegible] idea that a milk diet is prescribed on the principle of *half* [illegible]*ing* the patient in acute disease. It has been reserved for modern times to show that the liberal and often the nearly exclusive prescription of milk may be the best means, in not a few cases, of conveying adequate nourishment in sickness, when no other food can be assimilated. On the other hand, I am bound to add that although in Bright's disease and in diabetes an exclusively milk diet will occasionally, up to a certain point, succeed; I have found by experience in the latter disease that it cannot be regarded as curative, inasmuch as the loss of weight, when it is too persistently employed, shows that it is not really assimilated, even when not obviously rejected, or causing evident dyspepsia. In this respect my experience agrees generally with that of Sir William Roberts (*Urinary and Renal Diseases*, 1885, p. 300).

Note on the Thermometer as a Guide to Prognosis and Treatment in Typhus.—Although I have not made any alterations, in accordance with later experience, in the text of this lecture as originally published in *The Lancet*, it seems necessary to add here that the words used in p. 164, and more or less throughout, demand a considerable amount of modification, in accordance with the much increased, and much more habitually exact, use of the clinical thermometer of late years. I do not, indeed, find that the statements here made as regards the pulse are at all invalidated by the direct observation of the temperature; but rather, if anything, confirmed. At the time this lecture was delivered, however, I was not sufficiently impressed with the importance of very frequent notations of the temperature in acute diseases generally, though fully persuaded of th[illegible] value in tubercular cases. The much more adequate t[illegible]ing of our nurses is now such that, in hospital pract[illegible], and with skilful nurses who can be trusted, there is no longer any room for hesitation in giving observations on temperature the first place; and even requiring these to be made many times during the day and night in all very serious and acute cases. Instead, therefore, of being "cumbrous," etc., as a means of investigation, I now regard this frequent use of the thermometer (every three or four hours, as a rule) as part of the regular arrangements of every well-appointed sick-room, and desire the remarks made above to be revised in accordance with this rule. Very shortly after this lecture was delivered, indeed, Dr. Russell was kind enough to carry out, at my instance, an extensive series of observations simultaneously of pulse-notes and thermometric observations, both rectal and axillary, in the Glasgow Fever Hospital, then under his charge, with the result of showing that in typhus fever the facts stated above were essentially corroborated in every particular; the two means of investigation going well together in the great majority of cases, and accidental errors being thus more easily eliminated. But while thus fully adopting thermometry as a most valuable, perhaps the most valuable, clinical apparatus of our time, I am not at all converted to the uses made of it in Germany in respect of treatment; whereby methods in the highest degree disturbing, to say the least, have been brought into operation, in my opinion with questionable advantage and success, as compared with those here advocated. (See in *Glasgow Medical Journal*, article on "The Antipyretic Treatment of Specific Fevers," September, 1878; with subsequent correspondence, November, 1878; see also above, pp. 91, 92.)

V.—TWO MONTHS OF FEVER DUTY IN THE GLASGOW ROYAL INFIRMARY.

[Prefatory Note.—The paper, of which the leading conclusions are here given, was published in detail in the *Glasgow Medical Journal*, January 1865, where it occupied an amount of space that cannot be spared to it in the present volume. Nevertheless, at the risk, perhaps, of some apparent repetition, it is considered expedient that observations thus deliberately and carefully made and recorded during a great epidemic of typhus, having been published at the time specially in order to carry the kind of conviction that can only spring from the details of individual cases, should not be without some notice here, such as may induce even a few persons interested in the subject to refer to the original memoir. Some of these cases are individually interesting, in their bearing on the questions discussed in the preceding chapters; some of them are unusual, if not, indeed, rare. I have the satisfaction of knowing that all of these came under the scrutiny of the late Dr. Murchison; for no single incident of importance sufficient to require notice in my two months' summary has been omitted in the references or in the text of the second edition of his great work. To those who are familiar with the admirable thoroughness and the general freedom from prejudice characteristic of Dr. Murchison's writings, this fact is a sufficient assurance of his having maturely considered the whole details and the conclusions referred to, and it is on this ground chiefly that I think it necessary to take note of one or two points in which the second edition (1873) differs from the first (1862) in connection with the peculiarities of the crisis or defervescence in typhus fever. It is, perhaps, noteworthy that in both editions he adopts, as the basis of his doctrine of *crisis*, a description, not in his own language, but in that of the late Dr. Stewart, writing in 1840.[1] My regard for Dr. Stewart is probably not less than was Dr. Murchison's, and I am not prepared to affirm that this description may not have been, with reference to the fevers of 1840, as exact as its adoption by Dr. Murchison would presuppose; but in one point, at least, it certainly fails to correspond accurately with my own observations on the typhus of 1863-64, and indeed with epidemic typhus generally as witnessed in Edinburgh and Glasgow, from the time of the extinction of the great epidemic of 1847 onwards (I do not feel able to speak with critical accuracy of any earlier date than 1855 or 1856). "To such cases as these," writes Dr. Stewart, "we might almost apply the Scripture phrase: 'At such an *hour* the fever left him.'" I cannot doubt that this strong expression, thus becoming, as it were, classical in the comprehensive and universally approved work of Dr. Murchison, has given an erroneous bias to the description of others, if not of Dr. Murchison himself, here and elsewhere. It is needful to point out, accordingly, that such very sudden events are certainly not characteristic of typhus as I have observed it, though eminently so of the relapsing fever of 1843 and 1847,[2]

[1] *Edinburgh Medical and Surgical Journal*, October 1840, p. 305.

[2] I have a very vivid remembrance of the case of a young man at that time (probably 1847 or 1848), a colleague on duty in the

and also of the tertian and quartan agues from which the experts of Greece, from Hippocrates downwards, and, no doubt, the writers of the New Testament, obtained the idea of *crisis*. Even in Dr. Murchison's first edition, at p. 133, the fact as regards typhus is correctly stated in these words :—"After the first week or ten days, *the temperature usually falls gradually till convalescence*;" although it is true that in other passages of the same and of the second edition there are approximations to the typical description of crisis, tending to qualify this absolutely accurate statement, and in doing so, to place the *date* of the crisis (as I think) too late, *i.e.*, on or about the fourteenth day (the critical day of the second week according to the ancients). "This sudden fall of temperature *about the fourteenth day is peculiar to typhus* and may be useful in diagnosis" (second edition, p. 137)—is one of the strongest of these expressions, and one which can only be accepted as accurate, in my opinion, by omitting the word "sudden," and by taking the fourteenth day as the close, rather than the beginning, of the defervescence. At page 184 of the second edition, after quoting Dr. Stewart, and also Jacquot and Barrallier, Dr. Murchison writes as follows :—"These statements have been confirmed by careful thermometric observations. Although the acme may be attained in the first week, *and after this there may be a gradual fall*, and although, as Gairdner has shown, there

Royal Infirmary of Edinburgh, who in an attack supposed to be of exanthematic typhus, had an early and a sudden favourable crisis of the kind familiar to us in the relapsing fever, then also under observation side by side with typhus. The occurrence of a sweating crisis of this kind, with a pulse falling to normal, and a convalescence established within a few hours during the early part of the second week of typhus fever, was at the time regarded as so very unusual as, notwithstanding the eruption, to have given rise to many doubts and discussions as to the diagnosis.

may also be a gradual fall of the pulse in cases which recover extending over several days, the *final* defervescence, according to Wunderlich (whose correctness on this point I have tested by numerous observations) is usually 'precipitous.' (See Diagrams IV. and V.)" Now, on turning to these very diagrams, and to two others which are given along with them in Dr. Murchison's work (second edition, p. 138), I find that in Diagram IV., the crisis or defervescence takes place by quite a series of falls, extending from the eleventh to the sixteenth day, at which date nearly normal temperatures (with slight variations extending to the twenty-seventh day) are reached; in V., a decided fall takes place on the twelfth day, and another and greater one on the fourteenth, extending to the sixteenth day; in VI., a pseudo-crisis may be recognized even during the first week (sixth to eighth day), and then a succession of falls from the twelfth or thirteenth day on to the eighteenth or nineteenth, followed by recovery; and, finally, in the remarkably severe and fatal, though rare, instance selected from Wunderlich, an almost gradual defervescence from the unusual acme of 106·4° at the eighth day, *extends over at least a week;* the crisis, however, remaining incomplete with a minimum temperature of 102·2°, succeeded on the sixteenth day by a very sudden rise, and by death on the seventeenth day, with the extraordinary temperature of 109·2°. This last instance (confessedly a very exceptional one) is in accordance with what I have stated in a note, *supra*, p. 177, as to occasional and disastrous variations from the *norm* of typhus; but of all the others I contend that the defervescence or crisis (if so it is to be called) is altogether out of accord with the conventional description adopted by Dr. Stewart, and indirectly by Dr. Murchison; and that the true reading of the thermometric indications, as given above, is to consider the favourable change as extending from the eleventh or twelfth day onwards, and as being on the whole gradual rather than sudden. It was to some extent with the view of illustrating this point as bearing upon prognosis (though dealing chiefly with the pulse), that the cases referred to in the paper in the *Glasgow Medical Journal* were published; and from numerous records of temperatures afterwards procured from the Fever Hospital, while under the personal charge of Dr. Russell, I am able to affirm that in his experience also, at that period, the defervescence might be taken as mostly extending from about the twelfth day. Notwithstanding, therefore, my confidence in Dr. Murchison as a rule, it seems necessary, in view of the facts, to claim some modification in the terms of his references to this subject, especially at pp. 137, 184, and 187 of the second edition.

The cases referred to in the present chapter extended in a regular series over two months, September to November, 1864, and were, in fact, those which formed the foundation of the remarks in Chapter IV. of this volume. A sufficiently exact *coup d'œil* of the whole series, for practical purposes, will be found at pp. 159, 60, above. Although, therefore, a much more vivid impression as regards the facts above alluded to, and also as to the details of prognosis and treatment, may be derived from careful reference to individual cases and the clinical notes upon them as made at the time, it is only proposed here to introduce the more general statements and conclusions, referring to the *Glasgow Medical Journal* for further information and evidence of what is here set forth. At a time, moreover, when the reaction against what the Germans are now calling "nihilismus" is tending to compromise most of the principles here advocated, I may be permitted to point out that although these results have now been nearly a quarter of a century before the public, no one has deliberately attempted to show that *in typhus fever* (*i.e.*, *exanthematic* typhus) proved to be such, and treated by any more apparently active routine than is here indicated, results any better, or even so good as these can be definitely established; although it would appear to follow, from the principles on which the anti-pyretic treatment is based in enteric fever, that a system of very active interference is indispensable alike in all diseases whatever attended by considerable elevation of temperature. The disappearance of typhus fever as a wide-spread epidemic among us, and the segregation of such cases as have occurred, in the hospital of the Local Authority, have deprived me of any personal opportunity of pursuing the subject during the last twenty years. I am therefore the more desirous that the experience here recorded should receive its due weight in any future investigation.]

THE following general statements are applicable, more or less, to the whole of the cases, whether in the male or female department, and therefore need not be repeated. I have noted as "typhus," only cases presenting what is now so well known as the typhus rash, or, in default of this, cases so closely associated with others having the rash, as to leave no doubt of the character of the fever. A somewhat similar remark applies to the cases noted as "enteric fever," and to the single case of scarlet fever. Under the head of "febricula" I have been compelled to include one or two cases which *may* have been typhus, but in which no conclusive evidence existed of the character of the disease; and in one of the instances noted as "pneumonia," in the male ward, a possible doubt may perhaps be admitted as to the pre-existence of typhus fever not evidenced by eruption. The diagnosis of these various diseases was in all cases established and noted as soon as possible after admission, and in no case can the diagnosis be said to have been contingent on the event of the disease, or left to be settled by post-mortem examination or otherwise, after the patient's death or dismissal. The reader has therefore before him, as completely as is possible in a printed record of moderate length, the entire materials on which he may form his own judgment.

The treatment, where not otherwise specified, has been conducted mainly on the following principles:—To afford a bland and nutritious aliment throughout the fever, chiefly consisting of milk and water, butter-milk, rice and milk, arrow-root, and beef-tea, sometimes with a little bread where it was relished. The choice of the special form of diet was, to some extent, determined by the feelings of the patient, and to a like extent left to the discretion of the nurses, but milk diet, in all instances, formed the staple nourishment. Farther, it has been considered expedient to employ no medicine *as matter of routine*, so that, where no statement to the contrary is made, it may be presumed that no medicine at all, unless it may have been an ordinary laxative, or a little common cough-mixture, or the like, was administered throughout the disease. A similar remark applies to the administration of alcoholic stimulants, except that I have thought it right, in view of certain questions recently under discussion, to state distinctly, as from the hospital wine and spirit roll, the whole amount of stimulants given in each case; and where none were given to make an express statement to that effect.[1]

The *course* and *symptoms* of the fever which has formed the great mass of the epidemic, have, on the whole, been easily identified with those of typhus, as described by the best authorities of modern times. Yet there are not wanting differences of detail, as compared with the ordinary accounts of typhus, which, whether indicating peculiarities of the present epidemic, or differences of treatment, or both, deserve the attention of practitioners. Some years ago (see *Clinical Medicine*, p. 164), I remarked upon a change, which I had myself observed, in the *course* of typhus fever, and especially in the phenomena of the crisis, as compared with my own earlier recollections, and with those of my friends who were engaged in the great epidemic of 1847-48. Writing in 1859, I used these words:—"I have again and again seen, of late, the pulse coming down several beats, the eruption fading, and the tongue cleaning progressively, at every period between the tenth and fourteenth day; and, in the case of children and young persons at least, I am certain that the change has begun quite as often before the twelfth day as after it. I have even observed the favourable change as early as the very beginning of the second week, and had, at one time, learned to look upon the eleventh day as, on the whole, the one most frequently critical" (p. 165). Again, in regard to the character of this habitually early crisis, I remark, a little further on, "It (the crisis) is rarely quite rapid or sudden, usually extending over two or three days, and often barely appreciable till it has been forty-eight hours, or more, in progress" (p. 167). These observations, which many of my friends, both in Edinburgh and London, received with evident doubt and hesitation at the time, are fully borne out by the facts of the present epidemic, as here faithfully recorded. And I appeal with confidence to the observations of those who will follow out the variations of the disease and especially of the pulse, with the necessary care and exactness, for corroborative proof of this early crisis.

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[1] It appeared more trustworthy to make the statement as to the stimulants in this form than in the shape of an extract from the case-books, as I find that among the necessarily rapid records of so many cases there were many unavoidable omissions in the journals in this particular.

But observe the conditions of the experiment. *The patient must be in a very well-ventilated ward, with abundance of milk and other light nourishment, frequently offered; and either with none, or with very small and carefully regulated quantities of alcoholic stimulants.* I do not indeed venture to affirm, though I strongly suspect, that the opposite practice will lead directly to a delay of the crisis; but if any one shall fail, after due precautions, to observe the phenomena here alluded to in a large proportion of the cases of the present epidemic, I confess I should strongly recommend him to look to the wine and spirit roll, and see if the cause of the disturbance is not to be found there.

I will add to this one further remark, which is to be regarded as the corollary of the other, and of great importance whether as regards prognosis or treatment; viz., *that in the event of a gradual fall in the rate of the pulse being observed, at any period from the eighth day of the fever onwards to the fourteenth, the delay, even for many days, of the other phenomena of the crisis is not,* per se, *dangerous, and the result may usually be awaited without any change of treatment.* If the practitioner can but get a firm faith in these principles, and if he will apply them so as to keep his practice in accordance with nature, and not to pervert the course of the disease at this critical period by injudicious interference, I fully believe he will have done his best to reduce the mortality to a minimum.

The proof of these remarks is to be found in the whole body of the cases recorded briefly above, and in the more detailed narratives of them in the hospital journals. More especially I would refer to the following cases, in all of which the maximum rate of the pulse was definitely ascertained to have been attained about, or before, the 12th day:—Males, Cases 1, 4, 6, 11, 13, 18, 19, 21, 24, 28, 30, 33, 34, 35, 36, 38, 40, 42, 43, 44, 46, 48, 49, 51, 53, 55, 57, 61, 62, 63, 64, 65, 67; Females, Cases 5, 6, 8, 11, 12, 17, 20, 22, 27, 33, 38, 41, 44, 45, 46, 48, 53, 54. In several of these cases the pulse-maximum was as early as the 8th, 9th, or 10th day, and in the most of them the crisis in other respects was hardly appreciable for several days after the fall of the pulse; in a few there was even protracted delirium, a very dry tongue, and a persistent deep-coloured rash, for many days after the favourable change in the rate of the pulse was observed; but in not one of all these instances was there any real cause for serious apprehension after the decisive phenomenon above referred to, and in very few, if any of them, am I disposed, on a retrospect of the whole, to think that stimulants or any other active remedy was required. In a few of the more protracted crises, on the other hand, as in Case 4, and perhaps as in Case 12 (Females), I was led to believe (but, it may be, wrongly) that stimulants administered early in the disease, and to an unnecessary, if not injurious amount, may have had something to do with the prolongation of the fever.

With a few words, necessarily brief, upon the subject of treatment, I must close this too lengthened paper. The first lesson that has to be learned in dealing with fever, is at the same time to many minds the most difficult—to let well alone. And those who naturally or by education incline to a more or less active routine practice, are not, I believe, likely to be convinced by statistics, or by any form of abstract reasoning, that such practice is either unnecessary or injurious in such a disease as typhus fever. The argument, to many an irresistible one, will always recur—What! are we to stand by and to do nothing at all, when the pulse is 120, the tongue dry, the brain oppressed and delirious? and it may even be admitted that the question is an extremely embarrassing one for all who are not prepared to answer it out of a matured and carefully studied experience. But in view of facts which have become clearer and clearer in proportion as our knowledge of typhus fever has been defined and rendered more exact, it has become a duty with teachers of medicine, not indeed to acquiesce in a blind expectancy, any more than in any other unreasoning routine, but to take a firm hold of principles based upon experience, and to place these in the light of such practical illustrations as their opportunities, derived from hospital practice, enable them to command.

I would therefore invite the attention of all physicians who admit any doubt as to the proper practice in typhus fever, and especially of all who have still to attain practical experience on a large scale, to the cases here recorded, with the view of showing—

1. That at least two thirds, more probably three fourths, perhaps even a greater proportion, of the cases of typhus fever in the present epidemic, may be successfully treated without either medicine or stimulants; or, at least, with only such slight accidental doses of either as cannot be supposed to influence considerably the result of the fever, as such.

2. That, in the cases of typhus fever recorded in this paper, the treatment, whether by stimulants, or medicines, or both, has been directed entirely to emergencies and casualties, and not to the subduing of the febrile excitement, or the restraining of the typhoid delirium, or the reduction of the frequency, or increasing the strength, of the pulse; on the principle that fever, with rapid weak pulse and a certain amount of typhoid delirium, are to be regarded as the normal facts of the disease, always observed within certain limits of time, and not to be avoided, or cut short, by any known specific treatment; from which it follows, that remedies, especially very active remedies, are to be reserved chiefly for manifest variations from this normal course; as when tartar emetic has been given, in small doses with wine, in numerous cases of bronchitic complication,

or wine and whisky in obvious tendency to collapse; or, to take a less extreme instance, a purgative in accidental constipation, an emetic in gastric irritability, limewater in diarrhœa, or an opiate in case of protracted sleeplessness in the early stage.

3. That, under a practice so regulated, the mortality of typhus fever has been not more than 9·3 per cent. in the cases here presented, which have been treated absolutely without selection, and without excluding a single fatal case on any ground whatever.

4. That in children and young persons, probably up to the age of 20, or at least 15, typhus fever so treated is a disease of hardly any. danger.

5. That in all cases where a regular daily note of the pulse shows a gradual climax, ending in a maximum rate on the 8th, 9th, 10th, 11th, or 12th days, or even later, and afterwards a gradual decline in the rate, there is no occasion for alarm after the crisis of the pulse has been reached, even although delirium should go on, hardly abated, for several days; and that no sudden crisis is to be expected, or wished for, in such circumstances.

6. That nourishment, of the kinds indicated at the beginning of this paper, is to be carefully administered throughout the course of typhus fever. That free ventilation is also indispensable.

7. That good nursing, and constant watchfulness against casualties; especially watching of the chest, the

evacuations, and the skin of the back, are the real "physic" needed by all fever patients; and more particularly by the old, the intemperate, and the subjects of chronic disease, in whom the danger is incomparably greater than in the young, healthy, and temperate.

VI.—ON THE LIMITS OF ALCOHOLIC STIMULATION IN ACUTE DISEASE.[1]

IN a considerable number of detached papers, ranging over the last ten years, I have made allusion, more or less in detail, to the administration of alcoholic stimulants in the hands of physicians.[2] In one of these papers, in particular, the subject was rather elaborately and specifically discussed in its relation to the epidemic typhus of 1863 and 1864, as observed and treated by me in Glasgow. The discussion, thus limited to one form of disease, was considered to have a special interest, on the ground that "typhus fever is the disease of all others in which the practice of stimulation has been most largely followed;" and further, that "typhus fever, as marked by the eruption, is a remarkably well-defined disease, in which, with proper securities against error, there is likely to be but little difference between the diagnosis of one well-informed hospital physician and another." On the present occasion I propose, not so much to attempt any new course of research, as to reproduce some of the conclusions of the preceding inquiry in a somewhat different and less statistical form; extending, moreover, in some degree the basis of observation, so as to admit to consideration other diseases than typhus fever, and thus to encourage statements based upon the experience of physicians who may rarely have opportunities of treating typhus fever, either in public or private practice. It appears probable that the time has now arrived when this important subject should be freely discussed; and no occasion seems more suitable than the present, on which we see assembled practitioners from all parts of the kingdom, for the purpose of advancing and improving the healing art.

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[1] Read before the British Medical Association at Oxford, August, 1868.

[2] The earliest in date of these papers was a review published in the *Edinburgh Medical Journal* for February, 1858, on "The Duty of the Physician with respect to Alcoholic Stimulants." The second was one "On the Use of Alcoholic Stimulants in Hospital Practice," read before the Edinburgh Medico-Chirurgical Society in April, 1861, and afterwards printed in the above journal. Both these papers are included in a volume published in 1862, *Clinical Medicine, Observations recorded at the Bedside, with Commentaries.* The Glasgow experience, from 1862 onwards, is fully recorded in the present volume.

It may appear to require an apology that I should venture to bring into discussion a subject with which several names distinguished in physiological science have been associated, without adverting to the previous course of chemical inquiry as to the relation of alcohol, when ingested, to the textures and functions of the healthy body. This omission does not spring from oversight, but from a feeling of personal incapacity to follow out this inquiry, or even to indicate an opinion upon it in its present stage. Moreover, I have long been of opinion that in a subject having so many physiological, moral, and social aspects as the uses of alcoholic stimulants in sickness and health, some division of labour is necessary, and that the practical physician will, on the whole, be fully more likely to secure an impartial hearing for his own individual experience, if he determines, from first to last, to appear as a physician, and nothing more; *i.e.*, to decline the task of discussing, as the basis of his medical experience, the physiological and chemical theories alluded to, except, indeed, in so far as they necessarily arise out of his position as a clinical observer. For this reason, as well as on the personal ground above referred to, I shall desire to be excused if in the following remarks there should appear to be but scanty references to many of the vexed questions arising, and still, to my mind, undecided by science, as to the precise destination of alcohol when introduced into the animal economy, or its possible function in respect to the repair or waste of the textures. I would only point out (as has, indeed, been already done by my friend, Dr. J. B. Russell, in a work founded on the experience of the City of Glasgow Fever Hospital, and which is by far the most elaborate clinical monograph, so far as I know, that has ever been published on the subject of alcoholic stimulation in any one disease) that it is by no means essential to the argument in respect of the *clinical* use of alcoholic stimulants to have it previously decided whether alcohol is or is not a food. "All agree," says Dr. Russell (though I may add that exception may be taken to this proposition from a modern point of view presently to be briefly noticed), "that alcohol is a stimulant—nay, more, that its first and most evident action is that of a stimulant. It may or may not be a food; but whatever may be the truth in that respect, if not a stimulant it is nothing; if a food, it is a highly stimulant food."[1] In short, it is strictly correct to say that the *clinical* value of alcohol as a food (if any) is limited by its therapeutical action as a stimulant, because we cannot, under any circumstances whatever, develop the former property except in connection with the latter. It might, therefore, almost be maintained that the settlement of the exact nutritive value of alcohol is of no great importance from the practical point of view; but as I am desirous to avoid overstraining a conclusion in this direction, I shall only say that the physiological question may for a time be relegated to the purely scientific observer, while the physician will continue, as heretofore, to direct his administration of alcoholic stimulants by the observation of their more obvious effects on the nervous system and on the circulation.

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[1] *A Clinical Study of Stimulation in Typhus*, by J. B. Russell, M.D., Physician to the City of Glasgow Fever Hospital. Glasgow: MacLehose, 1867.

These remarks are necessary at the outset, not for the purpose of unduly assuming anything as proved on either side in the controversy that has of late years been maintained on this subject, but for the purpose of obviating an assumption that has been unduly made without proof, to the effect that alcohol not only *is* a food in the ordinary sense of the term, but that it is *the only* food that can be relied on to support the system in many states of disease. It is impossible to avoid observing that this assumption lies at the root of almost all that the late Dr. Todd wrote and taught on the subject of alcoholic stimulants; and in the wide influence exerted by his writings, not less than by his personal example, I must find my justification for thus referring to them. I should have thought it entirely unnecessary to disclaim anything like disrespect to the memory of Dr. Todd in so doing, had not something like a suggestion to this effect proceeded from my friend Dr. Henry Kennedy.[1] On this point, as on some others in the paper to which I refer, I must frankly say that I think my friend has spoken unadvisedly. I do not admit that it is even in the least degree ungenerous, or in the least degree disrespectful to the memory of Dr. Todd, to refer critically, but in strict good faith, and without malice or vindictiveness, to his published opinions. I think, on the contrary, that the true injustice to his memory would be in omitting so to refer to them. The instructions of eminent physicians are the milestones and finger-posts, so to speak, of our art. They are given to us for the express purpose of regulating and influencing treatment; and it is not too much to assume (in case they are worth criticising at all) that they have already contributed largely to guide and influence the opinions of men of less eminence and originality. In this particular instance it is notorious that the opinions and practice of Dr. Todd were, and are, very influential as regards the opinions and practice of multitudes of the readers and hearers of his instructions. It would be impossible to suppose, I think—at all events it never occurred to me to suppose—that a man so justly distinguished as Dr. Todd unquestionably was could have ever intended, in appealing to the opinion of the profession at large, that his views should pass without criticism, or even should be secured against further criticism by the fact of his death. I feel, therefore, fully justified in assuming that respect for his memory is fully compatible with the fair and public discussion of his opinions in this or any other professional assembly; and I have no doubt that even the most attached pupils and personal friends of Dr. Todd will agree with me in this view of the matter. Some of these thoroughly competent authorities will assuredly be both ready and willing to vindicate the truth of the doctrines transmitted to them, should they in any respect find these doctrines either imperfectly stated or unjustly attacked; and I believe that we may expect to be favoured, even in the present meeting, with the presence of several distinguished men whose relation to Dr. Todd was such as I have indicated, and who will be the first to admit that it is by frankly and amicably discussing the opinions of the illustrious dead that we can alone secure the fruits of their labours for the benefit of the living.

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[1] *Lancet*, vol. i., 1865, p. 6.

The root of error, as I believe—the opinion mainly responsible for the extreme lengths to which Dr. Todd and others have carried alcoholic stimulation—is to be found in the undue assumption to which I have referred above, viz.: that alcohol is not only a real food (which, hypothetically, I at least do not deny), but that it is *the* food, of all others, which is to be chiefly relied upon for supplying the material of repair in fevers; thus compensating the waste, and averting the functional exhaustion, characteristic of most acute diseases.

This doctrine I firmly believe to be erroneous; and if erroneous, it must not, in the great interests of humanity, be allowed indefinitely to shelter itself under the authority of great names, or to take refuge in the theoretical confusion which may be raised around it, in the present unsettled state of the physiological question. I shall venture, therefore, here to state the opposite doctrine in as precise terms as the nature of the case admits, viz.: that alcohol, *per se* (that is, apart from the other ingredients present in some stimulating liquors), is not in any sense a fitting material to be chiefly relied on for the repair of the textures in fever; that even admitting the doubtful hypothesis that alcohol may, under certain circumstances, act as a true reparative material, its practical utility in this direction is very strictly and even narrowly circumscribed by the stimulating or medicinal, narcotic, and ultimately poisonous, action which it always exerts upon the nervous system when given in sufficiently high doses; that in ordinary medical practice we cannot, as a rule, secure any considerable amount of the supposed *feeding* action of alcohol without a certain amount of this medicinal and poisonous action; and that the proper use of the stronger alcoholic liquors in acute disease is, therefore, therapeutic, and not properly dietetic. It is true that we here come in contact with a new element of confusion in definitions; for Dr. Anstie, in his late extremely ingenious, and in many respects valuable, work on stimulants and narcotics, seems to strike at the very root of the distinction between food and medicine altogether, and to maintain something like the doctrine that whatever, *in any way,* assists the living organism towards a just equilibrium of its functions, is, in effect, a nutriment as respects the special texture or functions on which it acts as a restorative. Without affecting to deny that there may be something to be said for this theory from the point of view of high and almost transcendental physiology, I think it must be conceded, in a practical sense, that there is a very cognizable difference between the mode of action, say of opium in peritonitis, or of arsenic and quinine in ague, and that of the more usual and natural restoratives of texture and vital function. I am not prepared, it is true, accurately to define a stimulant, and have no disposition to adopt any of those purely " vitalistic " theories connected, according to Dr. Anstie, with the term; but in the well-known effects of alcoholic stimulants I nevertheless recognize something much more allied to the effects of opium and quinine than of milk and beef-tea, and other alimentary restoratives, which may be administered almost according to the appetite of the patient.

Now, in order to bring the issue in the matter before us into the clearest possible shape, I shall maintain, as a doctrine based upon experience, that "alcoholic food," so called, is not only a bad and defective form of nourishment in most cases of fever, but (when used in quantity and continuously) a very positively injurious and disastrous one, inasmuch as it tends invariably, in the high doses prescribed by Dr. Todd and others, to destroy the susceptibility of the system for other and more natural food, to disturb the power of assimilation, to retard the elimination of effete matters from the blood, and by poisoning the nervous system, to render the patient more or less insensible to the natural wants of the exhausted body.

I should be sorry to appear to have over-stated the distinction between these two apparently contrasted arguments. Let me, therefore, appeal first to a few very plain facts as regards the practice resulting from the recognition of alcohol as a purely alimentary substance, and afterwards to the deliberate statements of theory by which these facts are supported.

In his *Clinical Lectures on Certain Acute Diseases*, there are numerous cases which are expressly put forward by Dr. Todd as illustrating the dietetic use of alcohol in fevers. From these cases I select two, partly because they are in relation to this subject recorded with more than usual minuteness of detail, and partly because the habits of the patients are either stated to have been temperate, or may be presumed from age, sex, occupation, etc., to have been so.

1. A nursemaid, 18 years of age (Case xxiv., M. A. Copstock), was affected with fever, pneumonia, diarrhœa, and afterwards rose spots. She was forty days, in all, under treatment, having been admitted on the 8th

day of the disease. During this period of nearly six weeks, it may be calculated from the record that she consumed at least 700 ozs., or about four gallons and a half, of brandy, viz.; 570 ozs. up to the 28th day of the disease, and a somewhat reduced and less definite quantity for some time afterwards. Between the 11th and the 16th day of the disease the quantities of brandy rose rapidly from 9 to 48 ozs. *per diem;* and, *notwithstanding a degree of drowsiness, which in Dr. Todd's own opinion* (as stated in the narrative) *seemed to indicate over-stimulation,* this maximum quantity was continued up to the 28th day of the disease. The details of the narrative in this case illustrate, in a very practical manner, the general doctrine held by Dr. Todd, and thus stated in a previous page of his work:—"I am convinced that it is much better to err on the side of over-stimulation than not to give enough.[1] In this instance he expressly states that a cautious reduction of the brandy from 48 to 36 ozs. in the day was attempted on two occasions; but these experiments were not considered satisfactory, and accordingly the attempts to reduce the stimulants "led only to their renewal in the previous doses."

2. A detective police officer (Case xxxiv., John Selby), "of temperate habits and previous good health," was admitted to hospital "in a state of almost complete

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[1] First edition, p. 130. The subsequent references are all to this edition.

coma," and "quite unable to swallow anything." From the description which follows there seems little doubt that the case was one of pure typhus, "with mulberry-coloured spots." Half an ounce of brandy was ordered every half-hour, in beef-tea, with other remedies for the comatose state. Two days afterwards it was discovered that the night nurse "had omitted to give him the brandy and beef-tea regularly." The stimulant was then doubled in amount, so as to reach 48 ozs. in the twenty-four hours. This rate of administration continued for three days, after which the former quantity was resumed. The march of this case was more rapid than that of the last, and in about two days more (seventeenth or eighteenth of the disease), something like a crisis occurred, when 24 ozs. of wine were substituted for the brandy, and continued during convalescence. The whole quantity of stimulants was about 250 ozs. of brandy, with an indefinite quantity of wine.

Now, in these two cases it is quite plain to the reader of the details that the administration of the stimulants to the extent mentioned was dictated solely by the fear of not being able to force in enough of the "alcoholic food" presumed to be absolutely indispensable under the circumstances. From the evident hesitation manifested, in the face of threatening coma, in reducing the daily allowance even when so high as 48 ozs.; from the uneasiness shown when it was discovered that the night nurse had failed to give half an ounce of brandy every half-hour, and from the immediate doubling of the dose, we derive a most vivid impression of the sense entertained by Dr. Todd of a certain paramount *necessity* which governed this part of his treatment. This appears still more clearly, if possible, from the case of John Gavin (xvi.), in which the question is discussed at some length; and it almost seems as though a doubt is raised as to whether the reduction of the quantity of brandy from 30 ozs. to 24 ozs. daily, in consequence of a state of continuous stupor, may not have been chargeable with the fatal issue. It is difficult to conceive of any state of mind, short of an absolute conviction that alcohol is *much more* necessary than ordinary food to a fever patient, which would justify such a method of administration as I have here indicated. It is a method which almost entirely disregards the previous habits of the patient; for it cannot be discovered from the record that a larger dose, or a greater total amount, of alcohol, was ever given, even to the most completely "seasoned vessel" than to the nurse-maid of 18, M. A. Copstock. And this case by no means stands alone: the rheumatic girl, Jane Cook (Lecture VIII., Case lxiv.), had six drachms of brandy every hour; the boy, James Jackson, aged 11, in pneumonia, had half an ounce of brandy every two hours, besides chloric ether and ammonia; and although the details are not fully stated, enough is stated to show that several gallons of brandy were given, in the course of only a few weeks, to a girl of the tender age of 14 (Lucy Wood, Case xviii.), far gone in organic disease. The whole drift of the argument of Dr. Todd's book is, that without such doses of alcohol as he describes it is impossible to pass safely through a fever of considerable intensity. Hence the alcohol (as he remarks elsewhere) must on no account be stopped, even though its use should appear to lead to flushing, or to increased delirium, or to coma; for it is more likely that the dose is too small than too large, and in any case, he remarks, "it is much better to err on the side of over-stimulation than not to give enough" (p. 130). Hence, also, the repeated cautions "against the *morbid* fear of over-stimulation, which leads many to adopt an opposite or vacillating course, and to allow their patients to die from exhaustion." "I must tell you, *in limine*," he writes, "that it is far more dangerous to diminish or withdraw alcohol than to give too much" (p. 484). Again—"I cannot too strongly impress upon you that, to do good with stimulants, you must use them early, with care and watchfulness, in very definite quantities, and not in a vacillating or timid manner. . . . The harm which they do (in disease) is grossly and unfairly exaggerated, and always due to the slovenly administration of them" (p. 270).

These statements are certainly entitled to the credit of being the fruit of a very strong and undoubting conviction. But are they well founded in fact? Is this large use of the stronger alcoholic liquors in acute disease so necessary? Are the dangers of their injudicious use to be found always, and alone, on the side of *under*-stimulation?

To both these inquiries I am led by experience to give a negative answer; and as, even apart from all theory, the statement of the facts is important, I shall ask your permission to place some of them before you in an extremely condensed form, referring to previous communications for the details of evidence.

As regards epidemic fever in the first place, I believe I have unassailably demonstrated that its treatment may be successfully conducted with allowances, even in the very worst cases, of stimulants far less in quantity, and far less continuously given, than those on which Dr. Todd was accustomed to insist from day to to day (as above shown), in the face of quite apparent symptoms of alcoholic coma, under the mistaken idea that life could not be preserved without going to the very verge of "over-stimulation." In a rather minutely detailed narrative of two months' experience in the Glasgow Fever Hospital,[1] I have accurately recorded 68 cases of febrile disease (chiefly typhus fever) in males, and 58 cases in females, in the precise order in which they occurred, omitting no single case (even when sent to the hospital actually moribund) that crossed the threshold of the ward; and yet I have calculated that the *whole of the* 68 *male cases* consumed only 328 ounces of wine and 401 ounces of whisky, being, in all probability, a less quantity of alcohol than was administered to the *one* young girl, Copstock, whose case is mentioned above. The mortality of these 68 cases was only 6—*i.e.*, less than one in eleven, one of the deaths being in a case of pneumonia, one in enteric fever, the rest in typhus. The number of these cases is of course quite too small for a statistical analysis of any value, and it is not my intention to insist upon this at present, further than to say that all the available evidence goes to show that the mortality was, as compared with almost any similarly and equally exactly observed series of cases, remarkably small, and that Dr. Todd's results, so far as they are known, were in fact very far inferior to mine in respect of *numerical* success. But the inference I wish to draw from these cases at present requires no such minute statement of results, nor any very critical consideration of their statistical aspect. Admit that the practice was only *moderately* successful—and this admission is quite sufficient to dispose of the theory that "alcoholic food" on a large scale is absolutely necessary for the safety of the majority of fever patients. It may be fairly presumed that in the list of 68 males of all ages, admitted successively, absolutely without selection and without exclusion, to a fever ward, in the very midst of a most severe epidemic of typhus, there must have been very many persons accustomed to the use of spirits, and not a few persons debilitated either by age or by the disease, to an extent that would have appeared to Dr. Todd to require the highest rates of stimulation mentioned in his book. Yet no one of these 68 males had more than 94 ozs. of whisky during the entire course of the treatment, and only four of them had more than 50 ozs.; nor was this deficiency compensated by wine, for only one case had so much as 71 ozs. of wine, and three others from 50 to 60 ozs., during the entire course of treatment. Among the females the total amount of stimulation was somewhat less than among the males, and it took more the form of wine than of whisky. Thus one female patient had 124 ozs. and another 100 ozs. of wine in the course of the treatment, but only one had so many as 60 ozs. of whisky. Further, it must be stated that in 49 out of the 68 males, and 34 out of the 58 females, there were absolutely no stimulants administered; and in many of the remaining instances the quantities given were so small as to be purely experimental, but at the same time such as to show that I did not, from any preconceived theory, or to carry out a dogma, willingly omit any reasonable opportunity of observing if stimulation appeared to do good. The details of the fatal cases leave it, of course, open to be argued that some of them might possibly have been saved by a larger administration of stimulants; but, on the other hand,

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[1] *Glasgow Medical Journal*, vol. xii.; *supra*, p. 193.

I was quite satisfied at the time, and am equally satisfied now, that if on this side there was a possible error, there was also, to a certain amount, an unnecessary, perhaps even injurious, administration of them to some of the patients who recovered. To those who recoil distrustfully from numerical arguments, I would recommend the study of these cases in detail. I believe that such a study of them will lead to the conviction that, under a habitual practice of high stimulation, the general result could scarcely have been better on the whole, while there is a good deal of reason to suppose that it might have been very much worse. Yet I should be sorry if any one drew the inference that I consider these cases as examples of a perfected or model treatment in all respects, or even that I look upon them as entirely conclusive with regard to the exact quantities of alcohol that may be most beneficially administered in fever. The only conclusion I wish to draw from them is, that the habitual, or continuous, or *alimentary* use of alcohol in fever is to be deprecated as at least unnecessary, and even to be suspected as possibly injurious.

But if this conclusion be not at once accepted as arising necessarily from the details of these recorded cases (126 in number in all) of consecutive febrile disease in two fever wards of the Glasgow Royal Infirmary, I have further to plead that the more extended experience acquired in Edinburgh and Glasgow since

1853 (when I began to act as an independent hospital physician) points unmistakably in the same direction. In order to make this position quite clear, it is absolutely necessary for me to refer to the name of another illustrious physician who has now left his memory and his career in the loving charge of the present generation. And if any one should think to accuse me of disrespect to the memory of Dr. Alison, I can only answer that my almost unbounded veneration for my old and dear teacher are well known, and that I have never on any occasion omitted to bear witness, in public and in private, to feelings which make even the suggestion of wilful disrespect simply out of the question. No one, however, understood better than Dr. Alison the sacred duty of maintaining what he believed to be the truth, and of showing forth open differences without the least breach of courtesy or of respect for those with whom he differed.

It was Dr. Alison's practice to commence stimulation during the first week, often as early as the fourth or fifth day, of fever, by giving about four ounces or more of wine or spirits in the twenty-four hours, according to the character of the individual case; increasing the doses gradually but steadily up to, and sometimes over the crisis, or until all danger from exhaustion or death by *asthenia* (as he termed it) was clearly past. No one who followed his hospital practice in connection with his oral instructions can fail to remember the theory on which this use of stimulants was based—viz., that in typhus fever, of the *type* observed in Edinburgh for several years before the period at which I attended his practice, *asthenia* was the constant danger, the ascertained fatal termination of such fevers, and that we must anticipate this termination from our knowledge of the type. Dr. Alison held that it was not sufficient to stimulate when asthenia had actually set in, or when the pulse began to show symptoms of failing strength. He maintained that even in the form of fever called, by Cullen, synochus (which he regarded as one in kind with typhus), it was necessary to stimulate *in advance* —*i.e.*, before any symptoms of debility or of typhoid affection of the nervous system, had appeared; because in such fevers, he said, we were practically sure that typhoid symptoms *would* appear during the second or third week, and therefore it was necessary to lay the foundation of a stimulant practice during the first week, while the pulse was still good, and the nervous system comparatively unaffected. The great care and solicitude with which Dr. Alison laid down these principles was no doubt due to the recoil in his own mind from the practice of an earlier date, at which fevers were commonly treated by blood-letting, and sometimes by repeated blood-letting in the earlier stages. Dr. Alison had been a witness to this practice, and up to a certain point (though never to the extreme point) had approved it; but he held that since 1829[1] (or some such period) fevers had not been observed such as would bear blood-letting; that the type had altered; that the practice had required to be adjusted to the altered type; and that even the most moderate blood-letting, under the altered epidemic constitution, was to be deprecated as tending towards the progressive asthenia, which it ought to be the object of the practitioner, above all things, to avert. The method of very early stimulation was the corollary of the doctrine of "altered type." The character of the fever being typhoid, the practice was to be such as would best ward off debility. "Obviate the *tendency to death*," he urged upon all his pupils; "stimulate your patient early, for the tendency to death is by *asthenia*, whatever the character of the pulse during the first week." It will be easily perceived that the practice of "feeding" fevers, so much insisted on by Dr. Graves of Dublin, was another form, and, as I think, a much better and safer form, of the same reaction in opinion. On this point Dr. Alison retained more of the hue of the earlier epoch than Dr. Graves, and his instruction was not so well defined; but Alison and Graves were, in fact, of the same school; the practice they adopted was essentially a protest against the starvation and depletion, the "idols," as Bacon might have said, of an earlier period; against the doctrine of "inflammation" as the essential cause of fever; against the ideas in vogue in 1819, and long lingering in the profession after the date of Dr. Welsh's famous book, to the effect that antiphlogistics in the early stages of all fevers were necessary, and even in the severest cases always tended to promote the cure.

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[1] See note p. 154.

The ideas of Dr. Alison were in full force, and his influence as a practical teacher was at his height, during the great fever epidemic of 1847-48. I well remember the practice of that terrible year in typhus, and have many a time ordered quantities varying from 8 to 12, or even 14 ozs. of wine or spirits per diem, in the second and third weeks of the disease. Sometimes wine was ordered at first, and afterwards spirits along with the wine. But I have no recollection of quantities at all approaching those mentioned by Dr. Todd, and am almost certain that I never had Dr. Alison's orders to administer even 20 ozs. of spirits in the day. I am under the impression that wine was given along with whisky on the theory that it contained more alimentary matter than whisky; at all events, the theory undoubtedly was that the alcohol, *per se*, was more a stimulant than an aliment. I am afraid that even with what we must now-a-days call the comparatively moderate scale of administration above mentioned, we not unfrequently witnessed " over-stimulation," in the sense in which I understand the term. I have a very distinct recollection of long-continued delirium, protracted crisis, and of a mortality which I should now, judging from twenty years' subsequent experience, consider absolutely startling, and which the records of the time stated at 22 to 25 per cent. How much of this may have been due to a special severity of the epidemic; how much to overcrowding of the wards; how much to destitution, the ultimate result of Irish immigration and the potato famine, I have now absolutely no means of ascertaining. But the result of the whole experience of that epidemic on my mind unquestionably was a growing scepticism as to the efficacy of high stimulation in typhus; and in 1855 (to go no further back) my own hospital returns show that I had adopted an extremely restricted administration of alcoholic liquors in general throughout the wards under my charge. The facts bearing upon this subject from 1856 to 1860 have been already the subject of a paper in the *Edinburgh Medical Journal* for May, 1861, afterwards reprinted in a volume, entitled *Clinical Medicine.*

On commencing hospital practice in Glasgow in the end of 1862, I found another great epidemic of fever already in progress. Four physicians were very soon at work in the fever wards; and I became aware, through various suggestive hints, that the *ratio* of stimulation I had insensibly adopted, after so many years of varied experience, was considered unusual, and even doubtfully orthodox, by some of the most respected of my senior colleagues. On one occasion it was pointed out to me, from the wine and spirit rolls of the house, that in the other wards of the fever hospital, at a particular date, fully six times as much wine and whisky were administered *per patient* as in mine; or rather, to state the matter more exactly, the wards might be considered as equally divided between two physicians who adopted a system of pretty high stimulation and two others who followed a practice of very great moderation in this respect. Under these circumstances it became a simple duty to inquire into results, and to secure, in so far as it could be done without invidious personal reflections, that the lessons of experience should be made as plain as the nature of the facts permitted. I accordingly gave the necessary instructions to my assistants, and I had the satisfaction of having them followed out with the most minute accuracy and faithfulness in detail. The result was a triple series of cases, amounting to 595 in all, which were analysed and commented on in *The Lancet*, March 12, 1864.

It is difficult to state here in a condensed form what I believe to be the just conclusions from this experiment, without laying them open to some more or less plausible objections; but the following facts are, I believe, unquestionable, and will serve to give an idea of the inferences fairly deducible from the whole.

In the year 1861 (commencement of the epidemic) before I had left Edinburgh, the recorded mortality of *typhus* over the whole of the Fever wards of the Glasgow

Royal Infirmary was 18·86 per cent. In the year 1862 it was 16·72 per cent. In both these years the ratio of stimulation practised was rather high, especially as regards the amount of wine; the proportion, however, cannot be exactly ascertained for the typhus cases separately, owing to the deficiency of data. In December, 1862, I became a physician of the Glasgow Royal Infirmary, but of course the statistics of that year can scarcely include any cases of mine treated to a termination.

In the years 1863 and 1864 the experiment above alluded to took place, and the number of cases biased towards a lower scale of stimulation amounted, I believe, to about one half of those under treatment. The mortality of typhus fever as officially rendered in the books for the whole hospital, was:—

In 1863, - - -	13·91 per cent.
„ 1864, - - -	13·16 „

In the succeeding years (under yet a different arrangement) the mortality was as follows:—

In 1865, - - -	15·81 per cent.
„ 1866, - - -	16·4 „
„ 1867, - - -	16·95 „

It has to be added that during the years 1863 and 1864, the mortality at all ages of the cases under my own personal charge (a few cases being omitted in summer, when the change of assistants for a time threw the returns into a slight degree of confusion) was as follows:—

First series, - - -	11·1	per cent.
Second series, - - -	19·0	„
Third series, - - -	10·3	„
Fourth series,[1] - - -	9·3	„
Total (amounting to 703 cases),	11·5	„

It thus appears that a total number of 703 cases, representing a very low scale, (possibly, indeed, *too* low a scale) of stimulation, gives a mortality of 11·5 per cent.; while the mortality from typhus in the adjacent years, in the same hospital and during the same epidemic, is from 15·81 to 18·86[2] per cent.

But by taking special account of the cases of the young (up to fifteenth and sixteenth year), the following very interesting results are obtained:—

In the two years of comparatively high stimulation (1861 and 1862) the mortality among the young in typhus in the fever wards of Glasgow Royal Infirmary was 3½ per cent.

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[1] It should be mentioned, also, that all the official figures taken from the hospital reports *exclude* the cases admitted moribund, which are all without exception *included* in my own four series.

[2] The fourth series was recorded with details in the *Glasgow Medical Journal* for January, 1865; the others were reported in the *Lancet* as above. In the second series the numbers were small, and many of the cases severe.

In the cases treated by me in 1863 and 1864, and corresponding to the four series of 703 cases at all ages, above mentioned, there were in all 210 cases (taking in those recorded in the *Glasgow Medical Journal*) below the fifteenth or sixteenth year of age; and among all these cases it is doubtful whether more than one was a death from typhus. It is better not to reduce this result to a percentage, however, as I believe it must be regarded, under any view of the facts, as a piece of surprising good fortune. But when we find that the mortality of children in fever in Dr. Todd's hospital practice was certainly considerable, and out of all comparison higher than this (making every possible allowance for imperfections of the record),[1] it is difficult to resist the conclusion that to the young, at all events, the administration of alcoholic stimulants in high doses in fever is fraught with a very serious danger from "over-stimulation." For although there are no cases of children in fever recorded with details in Dr. Todd's published works, private information leaves no doubt on my mind that his method of administration, as above described, was extended to the most tender ages, as well as to the most delicate and abstemious individuals of adult age.

Now, if these data be correct (and on this point I can only say that I have with the most scrupulous, almost religious, care endeavoured to keep them free from any error favourable to my own side of the argument), how can it any longer be pretended that alcoholic stimulation on a liberal scale is to be regarded as a necessary part of the *dieting* of fever cases? or how can it be reasonably maintained that even in severe cases of fever we must needs stimulate up to, and a little over, the extreme line of *tolerance* by the nervous system, on the ground that over-stimulation is a *less* dangerous error than under-stimulation? Let us take, for example, the cases of young persons recorded in my wards in 1863 and 1864, 210 in number. On the supposition that alcohol is the best, sometimes the only, kind of food available in cases of very severe fever, it is scarcely conceivable that there should not have been some cases absolutely requiring considerable doses of alcohol among such a number admitted without selection; some cases, in short, where not to stimulate was to abandon the only chance of recovery; yet only two little patients of all that number had even a single drop of wine or spirits, and these two only during a slow convalescence from cancrum oris, and only in quantities of two or three ounces of wine daily. Dr. Kennedy, who is a strong advocate of giving wine to the young in fevers, and who says in general terms (what I by no means dispute in particular cases) that "there is no class of cases of fever in which wine has a happier effect," does not venture to suggest that his mortality among the young in typhus fever has ever been *less* than mine, nor does he produce from the records of the Cork Street Hospital anything approaching to a list of 200 cases of typhus fever in the young (complicated and moribund cases *included*, observe) in which there has been a mortality of not more than *one per cent.* Admitting that I may have been unusually lucky (and this I am most willing to admit), is not the fact, nay, even the bare possibility, of such an amount of good luck, in the almost entire absence of "alcoholic food" in these 210 cases, a strong argument against the too common practice of giving stimulants habitually, or even frequently, and largely, among the young? In almost every outlying village, and certainly in every great town in this country, there are to be found many persons who still believe, with or without evidence, that wine, or even whisky, is almost a necessary part of the treatment of all fevers, even of those peculiar to childhood. It is surely unfortunate that such persons are so frequently able to appeal to the opinion and practice of eminent physicians as being without doubt or qualification in their favour, I have myself been repeatedly subject to adverse personal criticism, professional and non-professional, because I have steadily refrained from giving wine in fevers even where persons of influence about the sick were in favour of giving large quantities, but where the result has shown that everything was going on perfectly well. I have no doubt whatever that practitioners throughout the country in many cases succumb to this kind of social pressure; and as long as the authorities appear to be all on one side, they will be constrained to stimulate, with or without a reason. This may appear to be an unwarranted statement, but it is not so. I have had the confession made to me in confidence by a most excellent and conscientious practitioner that he hardly *dared* to abstain from giving stimulants in scarlet fever, because the whole of his neighbourhood had been led to think that such cases could not possibly go on well without plenty of wine or whisky. I have had most ample evidence before me, in consultations, that much injury—often, I believe, fatal injury—has resulted from the inordinate administration of stimulants in acute diseases, especially to the young; and therefore it has not been from any love of innovation or of paradox, but rather from a deep sense of personal responsibility, and under a most serious conviction of the importance of the subject, that I have been led gradually to the conclusions adopted in this paper.

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[1] See Murchison, *Lancet*, October, 1860: and compare Beale, *British Medical Journal*, October and November, 1860.

These conclusions, except as regards typhus fever, cannot even now be stated in any very strict or definite form; but the fair analogies deducible from the facts, and the gradually and insensibly acquired experience gained from watching individual cases of general acute disease, concur in rendering probable most of the following inferences:—

1. The importance of proper nourishment and sustenance in febrile cases is very great, and nothing can be worse than the old, and now happily exploded, system of depletion and starvation. But it is not less an error to conceive of alcoholic stimulation as a proper substitute, in such cases, for ordinary food. Milk is the normal food of most persons in acute disease, and there are very few persons, even in severe fevers, who cannot receive it to the full extent of their powers of assimilation; but most of the other simpler elements of an ordinary diet may be employed with advantage in particular cases. Beef-tea (so much favoured by Dr. Todd, in conjunction with alcohol) is of very inferior nutritive value, and therefore of altogether secondary importance to milk; and further, beef-tea very often disagrees with the stomach when milk and farinacea can be freely taken. In enteric cases beef-tea has been found occasionally to increase the diarrhœa, and it is usually to be avoided, or very cautiously given, on that account.

2. The proper mode of administering food in cases of febrile disease, is to imitate as closely as the condition of the patient admits, the usual practice in health; and most patients, even in severe fevers, retain so much of the habits and functions of health as to enable them to digest a good deal of simple food, conveyed into the stomach at proper intervals, and with sufficient intervals of rest to allow of a return of

the desire for food in some degree before again giving it. To give brandy and beef-tea every hour or half-hour, and to continue this practice for days and nights together, even wakening up the patient, (as is recommended by Dr. Todd) in order to give him his food and stimulant, is the surest of all ways to destroy whatever remains of natural appetite, and to derange altogether the already much weakened function of assimilation. In a few almost desperate cases, where the power of swallowing is nearly lost, it may be advisable to give food at much more frequent intervals during waking hours than in other cases; but rarely, if ever, to disturb the patient when asleep.

3. The proper use of alcoholic stimulants in cases of acute disease is nearly the same as in other cases, or in comparative health—viz., to quicken and develop the appetite for real food and the power of assimilating it. The proper and normal time, therefore, for giving these stimulants in fevers is *along with the food—i.e.*, at the *regular* meal times, and *not* during the intervals; except in cases where the nervous system is much disordered, and where a moderate dose of the stronger stimulants may sometimes be of great use in procuring sleep.

4. The proper dose of alcoholic stimulant, and also the proper kind of stimulant to give in each case, is subject to considerations derived much more from the age, sex, and previous habits of the patient than from

the severity or character of the fever. It is an error to suppose that patients affected with very severe symptoms of acute disease are capable of tolerating *indefinitely* greater amounts of alcoholic liquors than can be given in milder cases, or to the same persons in health. It is even probable that in some cases the susceptibility to over-stimulation is greater in fevers than it is in health. At all events, it is usually a practical error to carry stimulation much beyond the line above generally indicated.

5. In most cases of young persons, and in very many of adults not accustomed to the daily or frequent use of alcoholic liquors, these ought not to be given in quantity, or even at all, without the most clear evidence of their being really required. The effect of each dose should be carefully watched, and *it is almost always safer to withdraw the stimulant entirely for a period of hours than to run the risk of over-stimulation.* It is almost certain, from facts already observed, that in young persons the mortality of fevers is greatly increased by the *continuous* administration of alcoholic stimulants, even when the more striking or obvious symptoms of over-stimulation do not become apparent.

6. The effect of stimulants moderately and judiciously used in certain critical states of acute disease is to restore and refresh the patient, to favour natural sleep, to lower the rate of the pulse, to quicken the appetite, and increase the power of assimilating food.

The effect of stimulants continuously or immoderately given is to destroy the appetite for food, to produce torpor, to give rise to more or less profuse sweating, to dry the tongue, to prolong the febrile debility, and to protract the crisis. There is reason to think that in typhus fever the normal crisis may be delayed for several days by the constant administration of brandy, after the manner recommended by Dr. Todd.

7. In the self-limited fevers, in which the phenomena of the crisis are well known, and the course of the disease may be easily defined and anticipated, it is usually requisite to be very sparing of stimulants in what may be termed *normal* cases, because in these the disease is to be recognized as running a definite course, while any considerable excess has a tendency to convert the case into an abnormal one, and to postpone the crisis. It has already been sufficiently indicated that in the opinion of the author of this communication, alcoholic stimulants rank with drugs and medicines rather than with food. The same minute knowledge of the course of fevers which induces a well-educated modern practitioner to abstain from constantly drugging his patient will also, in most cases, lead him to abstain from stimulation. And, on the whole, it may be confidently anticipated, as a result of improved consideration given to the subject, that the profuse and continuous administration of alcoholic stimulants, with

a view to alimentation in acute disease, will ere long be abandoned as inconsistent with an enlightened physiology and a sound practice. While stimulants will continue to be prescribed to the sick, on the same principles and for the same ends as causes them frequently to be taken with refreshing and beneficial results in comparative health, their function in acute disease will more and more come to be recognized as a strictly limited one; so that *continuous* administration of them, or administration carried up to such doses as 24, 36, or 48 ozs. in the twenty-four hours, will be abandoned as involving an error in principle, and a serious danger to life in actual practice.

8. A subordinate, though still very important consideration, bearing upon such profuse and continuous stimulation as is above referred to, is that such liberal doses of wine and spirits given habitually under medical advice, tend to give a wrong bias to public opinion, and (even apart from the grave moral consequences arising from the abuses of alcoholic liquors) to involve the whole medical practice of the country in a system of unnecessary, and therefore wasteful, expenditure. It is as nearly as possible a demonstrated fact, that much of what is spent in wines and spirits for the sick in hospitals (and therefore probably also in private practice) is unnecessarily, if not injuriously spent. Typhus fever, as it occurs in Glasgow, almost always among

the poor, and often among the most ill-nourished, debilitated, and dissipated classes, is the very type of a disease which would appear to require the highest doses, and the most frequent and liberal administration, of alcoholic stimulants. Yet it has been clearly shown that typhus fever in Glasgow may be *so treated as to have a diminished mortality,* with the aid simply of milk diet and careful nursing; alcoholic stimulation being deliberately maintained upon a scale so low as to make it quite an exceptional, and therefore purely therapeutic, agent. In the City of Glasgow Fever Hospital, the careful and most scrupulously watched experiments of Dr. J. B. Russell[1] have led him almost insensibly and without any fixed plan, but *with an increasing success as regards results,* to lower from year to year the scale of stimulation on the whole, until last year the calculated amount of absolute alcohol administered in the form of wines and spirits to each patient, on an average, during each day of the treatment, was less than *one sixth of an ounce* (0·1630 oz.), and the expense of this quantity was little more than three farthings; while the entire bill for wines and spirits consumed during a year in which 969 cases of febrile disease (mostly typhus) were under treatment, was £34 5s. 6d. The mortality of typhus (including all the cases admitted moribund) during the same period was in this hospital only 9·05 per cent.; being the lowest ratio of mortality, I suspect, that has ever been attained in the treatment of so large a number as nearly a thousand cases, accurately distinguished as genuine typhus.

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[1] *A Clinical Study of Stimulation in Typhus,* Glasgow, 1867; and *Third Annual Report of the City of Glasgow Fever Hospital,* 1868.

VII.—MIND AND BODY.[1]

In taking the chair of this Association, allow me to offer to the members, whether present or absent, my sincere thanks for the honour they have done me in electing me their president. I frankly confess that I was not at all prepared for this honour, nor do I see quite clearly even now how it came about that a body of men devoted, both by personal tastes and by official position, to the cultivation of one particular branch of the great medical art and science, should have thought fit to honour with their confidence one whose relations with that special work are only those of the profession at large. At all events, it may justly be said that in having thus acted, you have most emphatically pronounced for the doctrine that the profession of medicine, and the healing art on which it rests, are *one*, and not manifold; and that as among the different churches, and even sects, we may hope to find a common Christianity, so among all the distracting specialisms of our own profession, we may reasonably hope to find one faith, one object, one discipline, and, to a great extent, one great *science*, both of mind and of bodily function, underlying all the diversities of our various careers as physician, surgeon, gynæcologist, oculist, aurist, alienist, etc.

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[1] Presidential Address delivered at the Annual Meeting of the Medico-Psychological Association, Glasgow, August 2nd, 1882. *Medico-Psychological Journal*, vol. 28, p. 321.

It will be my object to make this truth, which I believe to be a fundamental one, in some degree the theme, or motive, of the address which the custom of your Association, and your very great and unlooked-for kindness, has imposed upon me as a duty on the present occasion. But in doing so I must by no means forget what indeed I am forcibly reminded of by the letter in which your last distinguished president signifies to me his intention of being present at this meeting in Glasgow. "We come to Scotland," he writes, "determined to enjoy ourselves, and to 'throw physic to the dogs.'" If this little glimpse that I have accidentally obtained of the temper of your late president[1] represents at all truly the frame of mind and body proper to the Medico-Psychological Association to-day, it points out to me most unmistakably that there is one particular in which I may not even attempt to follow in his footsteps. No one can have forgotten the elaborate and most informing and eloquent address with which he favoured us last year, and which justly called forth the praise of the venerable Earl of Shaftesbury, bearing as it did upon the whole advance in the management of the insane, with which no names are more indissolubly associated than those of Tuke and of him who was once called Lord Ashley. No such address can you expect from me upon the present occasion. Even were I competent to give you "physic" for Dr. Tuke's "metaphysic" (as would be fitting for a professor of physic in the presence of a Medico-Psychological Association), I am bound to hold, with your ex-president's later mind, that both physic and metaphysic are to a great extent out of place when you are looking forward to a holiday on the Clyde; and when, to meet the exigencies of trains and steamers, the president's address must be restricted to the narrowest limits of time; when, indeed, the chief object to be served by addressing you at all is simply to avoid its being on record that the ceremony has been altogether omitted.

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[1] Daniel Hack Tuke, M.D., LL.D. See note below, p. 257. Dr. Tuke is (I believe) a descendant of the founder of the York Retreat, alluded to further on in this Address.

The duty, then, which stands before me—the question I am asking myself—is this: What can I say to you in this necessarily limited time that will in any degree touch a responsive chord in your breasts, to show that you, as being what the world call *specialists*, and I, as a professor of medicine in general, have in reality but one aim in view; that we are guided by one and the same scientific method and doctrine in dealing with the unsound mind on the one hand, and the unsound body on the other? To justify our belief that we are all one profession (which I hold to be indisputably the case), we must, in the end, be able to show that we work upon the same lines, with similar instruments both of research and of cure; that we aim, not at separating and dissecting the complex of functions which constitutes human nature into a bodily and a mental part; but that each of us, in his own separate sphere, is dealing with humanity as a whole, in which body and mind are inextricably interwoven. This I believe to be the true philosophy of the healing art, in all its separate departments; and whoever, even for a moment, forgets this, its essential unity, founded on the larger unity of human nature to which it ministers, has already gone some steps on the fatal road that leads to unworthy views of his profession. He has, in other words, already adopted a position which, when biased also by low moral proclivities and the love of money, soon gives place to quackery. We witness this degradation of the healing art every day in the case of those specialisms which, attracting as they have done some of the noblest and most active minds in our profession, have also become the pursuit of some who, it is not at all a harsh criticism to say, have become specialists only with a view to personal advantage. Hence, while even in the most extreme specialisms, the eye and the ear, we have had on the one hand such every way excellent men as Toynbee, and Hinton, and Mackenzie, and Dalrymple (to name only the dead), we have had, on the other, men who have been content to remain in complacent ignorance of the bearings of medical science at large on their specialism, if only they were allowed to cultivate it in a corner, as it were, for the benefit, not of humanity, but of the particular and individual man cultivating his particular and individual organ, eye, or ear, or skin, or spine, or womb, as the case may be.

You, gentlemen, are not open to these base suggestions. It is characteristic of your work that it has become a specialism, not through individual impulses or for individual gain, but through circumstances in the nature of the work itself, making it in some degree a public function, and thus guarding it in a large measure from becoming a prey to the vampires whose only business with the medical art is to drain its life-blood for their own particular use and advantage. Most of you are at the head of accredited institutions, where you are safe from all reproach, and can well afford to look down benignly on the men whose vocation it is to earn their chance guineas more or less honourably; but, if honourably, still with a trembling solicitude lest the contrary motives should ever be imputed to them. Although protected by happy circumstances, however, from the baser temptation of venality in the practice of your art, you are by no means secure against the bias inherent in all specialisms, legitimate or otherwise, to dissociate their cultivators from the general stream of tendency, so to speak, and from the actual facts and principles of the healing art in general. In some respects you are perhaps more than most others in danger of this. For you can, if you so please, live, and even live happily and well employed, quite apart from medical progress, and from general medical society. You have large administrative as well as strictly medical functions, and it would be by no means surprising were the former in a great measure to supplant the latter, and the treatment of the insane to become a specialism wholly divorced from the progress of medical science and of the medical art. It is not too much to say that whatever else is intended by it, the honour you have done me in placing me in this chair is a distinct recognition of the fact that such divorce is possible, and that you would regard it as a misfortune. You wish to draw closer the bond between psychological medicine and general medicine. You wish to declare somewhat emphatically that while you are, officially, devoted to the treatment of the insane, you have not ceased to be physicians; that in dealing with the unsound mind you are dealing not with the *mind* alone, but with the *man;* in other words, that you are doing exactly what I am doing every day—considering a particular ailment in its relation to the whole of the functions that make up mind and body, and treating it accordingly.

It is from this point of view that I am led to think that a few glimpses (for it can be no more) into some of the relations existing between your specialism and the healing art in general may be made interesting during the limited time at our disposal. But to such of you as have devoted attention (as I daresay we all have more or less) to the intricate questions arising out of cerebral and mental pathology as bearing on the cause or causes of insanity, the responsibility and capacity of the insane, and other subjects which grow out of the very depths of the philosophy of mind as related to organization, I have to say that I shall not to-day enter on any of these difficult questions. I am not, indeed, at all wedded to any theory of the association of "Mind and Brain," or of "Body and Mind;" but I hold it to be an unquestionable truth that the analogies, and even the laws, of bodily function shed an immense amount of light on the study of mental diseases, and *vice versâ.*

In seeking for evidence of this position I will not linger over points of recondite and doubtful pathology, but I will go at once to what must be regarded as one of the most brilliant advances of the healing art that has taken place within the compass of two generations, or even of a century—the improved, or, as it is often called by way of contrast, the *modern*, system of treatment of the insane. Taking origin in the last decade of the eighteenth century, this may be said to culminate in the present decade of the nineteenth, in such a large pauper asylum as Woodilee, which, at half an hour's distance from Glasgow, and easily accessible at all hours by rail, may be specially commended to your notice, as under the care and superintendence of our Scotch secretary, Dr. Rutherford. What is the essential basis of the great and beneficent change, which now-a-days not only permits of our dispensing with all the old miserable paraphernalia of physical coercion and restraint, but has advanced so far as to consider even locked doors, and bolts, and bars, and every attribute and reminder of prison life, as more or less an obstacle to the perfectly rational, and at the same time thoroughly humane treatment of insanity in our asylums? Or rather, let us say, what is the common principle, speaking of it from the physiological and medical point of view, illustrated in all the changes that make the difference between the Bedlam of 1815, whose manifold abuses, as laid bare by a parliamentary inquiry, are so well known to all of us, and the Bedlam of 1882, under the control and supervision of our friend, Dr. Savage? Religion, practical Christianity, the growing sense of humanity, the growing dislike to extreme severity, which have tempered our whole legislation during the lapse of this interval, have no doubt a large share of the credit; and it may even be said that reforms have been forced upon unwilling minds, medical and non-medical, by the same legislative impulses, the same moral necessities that have compelled the abolition of slavery, the gradual restriction of capital punishment, the improvements in the condition of our criminals, of our paupers, of our mining and factory populations, of our overcrowded masses in the towns, of our often ill-housed and ill-nourished agricultural labourers. The names of Howard, Wilberforce, Romilly, Chadwick, and Lord Ashley—now Lord Shaftesbury—stand out in strong relief as the persistent advocates of what may be called, without prejudice to their real importance and practical efficiency, a great series of humanitarian reforms. Lunacy reform was no doubt one of these, and it was taken up and forwarded by many active and generous minds, amply prepared through their previous training in other departments, and by the evolution, as it were, of the historic and moral conscience of humanity, to deal with the shocking abuses in the management of the insane as a matter of social, moral, and religious duty. But it is not to be forgotten that legislative action against gross abuses is something quite distinct from, although indeed largely due to, the firm grasp of principles which enabled William Tuke, and Pinel, and

Conolly, and many others to initiate and carry out positive reforms in moral treatment, far outstripping, alike in their designs and in their result, the course of legislation. The mere abolition of whips and instruments of torture, even had it been possible as a legislative reform apart from enlightened medical opinion, would have gone a very small way towards carrying out what we now recognize as a scientific, not less than a humane, treatment of the insane. It may even be affirmed with certainty that had the strait-waistcoat finally approved itself to the medical mind as a necessary and successful means of dealing with maniacal violence, no legislation could possibly have done away with it, however strong might have been the disposition, on grounds of humanity or of religion, to limit the application of severe and rigorous means in the management of the insane. Indeed, it is abundantly evident from the history in detail of the movement itself that neither religion alone, nor philanthropy alone, nor yet both of these acting together with such medical philosophy as prevailed during the greater part of the eighteenth century (and much more in the preceding ages), could ever have brought us to the point at which we now stand. What was wanted, and what has been, step by step, attained, was the application of strictly scientific principles of treatment, in accordance with an enlightened view of physiology and of human nature, to a class of cases in which, more than in any other, medical considerations had been subordinated to the mere impulses of fear, and the still more ignoble impulses of selfish greed and dark superstition, whereby the insane had been placed for centuries almost outside the pale of our common humanity.[1]

As if to show that, historically speaking, the improved treatment of the insane was not the result of any single or of any exclusively national impulse, we have the very curious fact that in the last decade of the eighteenth century the attention of Europe was startled by two perfectly novel experiments—the great reform at the Bicêtre by Pinel, and the closely corresponding, but perfectly independent, movement by William Tuke, which issued in the founding of the Retreat at York. No two men could possibly have been chosen out of all Europe at that time, of whom it could be said more truly that they were cradled, and nursed, and educated among widely differing social, political, religious influences—the one a member of the Society of Friends in England; the other a child, if not a nursling, of the French Revolution. Tuke had to work his way amid obstacles, no doubt, but in a moral and religious atmosphere predisposed to philanthropic ideas, and devoid of political passions. Pinel had to take his orders from Couthon, fresh from decreeing the abolition of monarchy and the sovereignty of the people, ere he could loose the fetters of a single lunatic in the Bicêtre. Yet in this particular matter Tuke and Pinel were possessed by one and the same idea, and worked it out as though it were an inspiration. And an inspiration it was without doubt, if we are to believe in an overruling Providence in history at all. For it must have happened often enough before that good men and true must have had misgivings, and even shuddered in their secret souls at the cruelties practised upon the insane. But now the fulness of the times had come, and by a twofold experiment, carefully and deliberately conducted in France and in England, a latent, and all-but-forgotten, truth was to be gradually brought into the full light of civilization, viz., that the unsound mind, like the unsound body, can only be regarded as an instance of disordered function; and that, however great the disorder, the functions are still there, and may be roused into more or less healthy activity by exactly the same physiological stimuli and motives as are available in the state of health. " *Vous voyez*," writes the Swiss physician Delarive, visiting the Retreat at York only two years after it was opened (1798), "*que dans le traitement moral on ne considère pas les fous comme absolument*

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[1] This was the leading theme of "The Morison Lectures" as delivered by me in June, 1879, before the Royal College of Physicians in Edinburgh; the first two lectures being in part reproduced, in substance, in this Address, and also, more fully, in a little work entitled *Insanity: Modern Views as to its Nature and Treatment*, Glasgow, 1885.

privés de raison, c'est-à-dire, comme inaccessibles aux motifs de crainte, d'espérance, de sentiment, et d'honneur, on les considère plutôt, ce semble, commes des enfants qui ont un superflu de force et qui en faisoient un emploi dangereux."[1] In other words, the insane, who had long been regarded as mainly objects to be restrained, governed, and as far as possible kept out of sight, nay, even regarded as "possessed," *i.e.*, *not* human, but in a very sadly real sense out of the pale of humanity, were to be deliberately and in the fullest sense readmitted within the pale, and even treated with more consideration than others on account of their infirmity.

It may seem strange in the present day that this lesson should have had still to be learnt in the nineteenth century; nay more, that it should have required the successive examples of Charlesworth, and Gardiner Hill, and Conolly, and perhaps of others extending down to our own day, to confirm and extend the humane principles affirmed by Tuke and Pinel, and make them thoroughly a part of the great heritage of our art. But it is to be remembered that mere selfish-ness and apathy and ignorance were not the only evils to be overcome; perhaps not even chiefly these, in the case of the medical, as distinguished from the more general, management or police of the insane. The theory of almost all diseases up to a comparatively late period was that they were *entities*, by which I mean a physical, or metaphysical, or metaphorical *something* (it is not always quite clear *what*, even in theory) distinct from the organization, and to be got at, and removed from it, only by disturbing remedies, more or less of the nature of antidotes to a strong poison, or at least equally obnoxious to the normal functions of the body. I do not think I shall be accused of overstating the case if I say that this misleading theory, greatly shaken, it is true, in the earlier part of the century alike by the progress of physiological science and by the extravagant claims of certain modes of practice, has been finally dislodged, as a general or working theory, within my own time; and no inconsiderable part of the credit in dislodging it is due to one who was closely associated with Conolly, and who, beyond all question, must have largely shared his ideas, and applied them in a different field of practice. In saying so much, I do not mean to affirm that everything Sir John Forbes wrote upon "Nature and Art in the Cure of Diseases" approves itself to my mind, but only that the analogies to which I alluded in the earlier part of this address as existing between bodily and mental disease are fully carried out in the history, even within modern times, of the most general principles of the healing art as applied to each. What I should incline to name as the most important gain of modern physiology and pathology for the healing art is that in lieu of the old conception or theory of disease as a separate *entity*, we have admitted largely this new one—"Disease is, for the most part, *normal function acting under abnormal conditions*." In other words, we do not admit habitually that a fever, or a sunstroke, or a stone in the bladder (to take a few random instances) is to be explained as a reversal, or abolition, or even a suspension of natural function, but only as an obstacle or impediment somewhere existing to the display of natural function after a perfectly normal manner. The normal function is *there*, still struggling, as it were, to assert itself (as long, that is, as the patient lives) even in the midst of the most extreme disorder caused by disease. And the problem of cure consists, for the most part, not in the administration of antidotes or the discovery of specifics, but in the careful study of all the details and modes of functional activity in the patient, with the view of removing obstacles, strengthening weak points, and, in general, assisting normal function to overcome abnormal, as far as may be. We have come to aim at treating not so much the disease as the man affected with the disease, administering our remedies not upon the principle of warring with an occult foe in some obscure corner of the organism, but on the far higher principle of dealing with the whole man, and assisting, sustaining, supporting all that is sound in him to overcome what is unsound.

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[1] I have great pleasure in taking this extract from a work which came into my hands only two days before the delivery of this address, and which is at once the most recent and one of the most valuable contributions to the literature of the subject, by the late President of the Medico-Psychological Association—"Chapters in the History of the Insane in the British Isles" (p. 117). By Daniel Hack Tuke, M.D., F.R.C.P., etc., London, 1882.

No doubt, although a modern, this is also a very ancient principle or view of the scope of the healing art, having been clearly enough formulated in the famous saying attributed to Hippocrates,[1] *νούσων φύσιες ἰητροί*, often wrongly interpreted (as I believe it was even by Sir John Forbes) when it is made to cover some quite abstract conception of "Nature" as a healer of disease. What Hippocrates really says is that "our natures are the physicians" (or healers) "of our diseases"—in other words, that normal function is in every instance to be evoked, and supported, and protected, as what is usually the only way open to us for effectually overcoming abnormal function. The increased scope given to this ancient maxim is perhaps the chief distinction of modern practice in all departments of the art, as in the healing of wounds and fractures by simple protective agencies and antiseptics; the substitution of nourishment for depletion in all forms of acute disease; the use of regulated exercise, and sometimes of electricity or galvanism, in paralytic and neuralgic disorders; the greatly increased employment of hygienic agencies both in acute and chronic diseases; and (as being closely allied with this) the whole field opened up, almost within the last quarter of a century, of preventive medicine. The use of such means now-a-days, in contrast with the endless bleeding, and purging, and starving, and vomiting, and other perturbative practices of a bygone age, is a strong testimony to a change in medical practice as applied to bodily diseases, closely parallel to that with which you are all familiar as having taken place in psychological practice.

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[1] We have seen, in the first chapter of this volume, p. 9, that this saying is, without question, attributed by Galen to Hippocrates himself; and therefore it was of great antiquity even in Galen's day—*i.e.*, older than the Peripatetic system, or than Aristotle, with whose philosophy, however, it might be supposed to have been associated. And Galen's mere opinion on this point is, for us, of very great importance, inasmuch as he was not only a most careful, but also a most critical, reader of the works of his predecessors, which he viewed in the light of an extended knowledge of them in detail, to which no one in the present day can have any claim. As it comes to us, however, in the fifth book of the "Epidemics," the saying is certainly post-Hippocratic in date. "*Vis medicatrix Naturae*" is a comparatively modern version, not nearly so close to the original as the one in the text, which is that of Dr. Latham, in his translation of Sydenham.

I would by no means be understood to argue that the abuses of the older medicine of the body, even at their worst, were at all comparable to the frightful injury inflicted upon the poor helpless lunatic delivered into the hands of his jailers in the Bedlams of the past. The evil there was that medical treatment, properly so called, was either wholly lost sight of or entirely subordinated to the principle of what was supposed to be safe custody. The few physicians who intervened in these abominable practices had their nature, "like the dyer's hand, subdued to what it works in," and were hardly a fair specimen of the medical profession, even in those days. Nor do I suppose for a moment that Pinel clearly foresaw the principle of the revolution that has taken place since his time in the theory and methods of medicine in general. Conolly probably perceived something of this, and certainly his friend Sir John Forbes must have had a tolerably clear notion that what had been done in the field of insanity had also got to be done as regards fever and pneumonia, making allowance for the difference of the starting-point in the two cases. The treatment of *delirium tremens,* first by bleeding and depletion, next by enormous doses of opium and by other narcotics and stimulants, last of all by a simpler and more natural method, affords an instructive instance of an intermediate department in which the same principles essentially apply. I have a vivid and abiding impression of the reaction produced in my own mind, at an early period of my career, by witnessing the disastrous results of the second of these methods in hospital practice, although at that time in full accordance with the teaching of most medical text books in this country. And as the *delirium tremens* wards in the Royal Infirmary of Edinburgh at that time were by no means devoid of strait-waistcoats, which the old nurse was not slow to employ in cases of difficulty, the evils both of mechanical and of what is now called "chemical" restraint were vividly presented to my mind before I was well out of my studentship, and long before the independent responsibilities of medical practice, and the mature consideration of the action of remedies, had led me towards the more excellent way attributed to Dr. Ware, of Boston, and introduced to this country in the pages of Sir John Forbes's Review. But the sad experience of private practice since that time has satisfied me that it is impossible to give effect to sound and safe principles of treatment in this disease, so long as the state of the law, and the want of proper institutions, obliges the management of such cases to be undertaken at home. Were it possible to send them all to Woodilee or to Gartnavel on the first appearance of the symptoms, many lives would undoubtedly be saved by the judicious employment of the ordinary measures of asylum treatment, with skilled attendants and ample space for exercise. But every one knows that this is not possible. The risk and the questionable legality of compulsory detention, the reluctance of friends and of the patients themselves to allow removal, and the absence (perhaps unavoidable under the circumstances) of proper accommodation in the ordinary hospitals for such cases, lead, I believe, to a considerable sacrifice of life annually in a city like this, and perhaps in all parts of the country, certainly not less, too, among the wealthier classes than among the ranks of the poor. This, however, is part of a very large subject, on which we have no time to enter at present.[1]

The sum of what I have endeavoured to place before you in the preceding remarks is that there is a substantial correspondence, if not identity, between the principles which have guided the modern improved treatment of insanity in our asylums, and those which now preside over the treatment of bodily disease in our hospitals and in private practice. The old and grim spectre of demoniacal possession, which has been responsible for so much cruelty and mischief, and even the more modern *mythus* of the *aliena mens*, familiar still in our law books, preserved also as a "survival" in the ordinary French phrase of "*aliénation mentale*" (which, by the way, was the title of Pinel's book), are not more opposed to the theory and practice of the asylum physicians of these days than is the old conception of bodily disease—as a separate morbid entity, to be grappled with and forcibly expelled only by "physic"—opposed to the whole scope and tendency of modern medicine. What you are familiar with in your improved asylums is precisely the principle that mainly guides the treatment of our hospitals and of private practice in the best hands, viz., that in the whole pathology of disease normal function must be held to underlie abnormal function; and that in the cure and treatment of disease accordingly the sound elements still remaining must be carefully respected; strengthened and built up again, if possible; in all cases, however, anxiously tended and nursed; the sound man within the unsound, the sane man within the insane, being supported and buttressed up, as it were, so as to reduce to a minimum the injury caused by the disease. This principle I hold to be, in the largest sense of the word, a *humane* one, in that it is carefully grounded on the consideration of human nature as a whole, and not taken piecemeal. Being so, it is also humane in the narrower sense, in respect that it does not allow any amount of diseased action or function, whether of mind or of body, to deprive the sick man of any of the privileges of our common humanity which can, in the very nature of the case, be accorded to him. And all the questions of capacity and responsibility of the insane accordingly take their colouring from the conception that there is no absolute line of demarcation to be drawn between the insane and the sound mind—that it is not *aliena mens*, but a mind having like passions and emotions with our own, only perverted and obstructed by disease, which is also disordered function.

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[1] In the author's *Clinical Medicine*, published in 1862, p. 259 *et seq.*, the whole subject was set forth on the basis of particular examples, and of the experience acquired at that date, which, in the main, is confirmed by all his later experience and reading.

VIII.—SANITARY SCIENCE AND PREVENTIVE MEDICINE.[1]

In the course of the duties which fall to my lot as a teacher of the healing art, I am obliged, as it were, to appear in two characters, because the art itself falls practically and conventionally, if not naturally, into two divisions—separate in idea, and yet, as we shall presently see, inextricably interwoven. "How is this individual patient to be cured of his actually existing disease?"—is the first question to which the art of medicine in all ages has invariably and inevitably addressed itself. How is disease—*i.e.* premature death, disabled faculty of body and mind—to be reduced to a minimum, and as far as may be got rid of, for humanity at large?—this is a second and a far more comprehensive question, in the course of answering which, when he attempts a reply at all, the physician hopes chiefly to get some additional light as to how to deal with the first. In addressing himself to the first question, the physician is led to view diseases simply as so many separately existing facts or species which have to be classified and set apart like objects of natural history, in order that the experience of many men in many ages may be brought to bear on the great question of their cure. And although there have never been wanting philosophic minds which have looked behind this conventional view of a disease, and endeavoured to obtain an insight into the causes of disease in general, yet it must be admitted that the habitual attitude of the physician towards his patient in all times has predisposed him to accept empirically, as it were, the disease as an existing fact, and to direct his whole energies towards the question how the individual patient, being already ill, can best be treated so as to emerge whole and sound.

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[1] Presidential Address, delivered in Section I. of the Sanitary Institute of Great Britain, meeting in Glasgow, September, 1883. In moving a vote of thanks to me as President of the section, Professor Humphry of Cambridge, the distinguished President of the Institute, is reported, amidst personal compliments which are here omitted, to have spoken as follows:—"It was not to be expected that ever again in the history of sanitary science would they hear an address delivered by one who held the important and engrossing position of Professor of Medicine, and yet had devoted so much of his time to medical sanitary science." To which I will only add—"*Absit omen!*" On grounds stated in the address itself, I trust that this anticipation may not be fulfilled.

Prevention of disease, the great object of modern sanitary science, has too often been regarded as unprofitable: not merely in the coarse sense of being unremunerative, but also in respect that the inquiry into causes belonged for the most part to the class of speculations condemned by the inductive philosophy as destitute of fruit, and therefore wholly insecure and unpractical. Causes were scholastically divided into predisposing, exciting, and proximate; but of these the last came to be regarded as an idle and metaphysical research, while the others gave rise to long dissertations, not without value, but certainly in most cases without much precision of method or obvious benefit to mankind. In very many instances, accordingly, the physician who applied himself to the study of causes was adjudged to be only losing sight of the object for which he was consulted by suffering humanity; while he who directly applied himself to the cure, and to nothing else, was glorified as being alone, in the real sense of the word, the mediciner, *i.e.*, healer or practical man. And the rewards of professional life being mostly reserved for services of this kind rendered to individuals, the inquiry into causes was necessarily discouraged, and disease came to be largely dealt with as a "visitation of God"—inscrutable in its nature and essence, but yet requiring to be attacked and subdued by remedies, many of which were violent, empirical, and dangerous in their action, in proportion to the supposed danger of the disease.

It was impossible, however, that this one-sided view of the nature of diseases should satisfy all minds—or indeed the best minds—devoted to the healing art in any age. All through the history of medicine we can observe the effort to get insight into causes, not merely as a philosophical speculation, but with a view to prevention. The treatise in the Hippocratic writings on airs, waters, and places is one of the earliest, and certainly, in its time, one of the most surprising advances in this direction. The curious and instructive studies of epidemic diseases in the same group of treatises, partly by Hippocrates himself and partly by his successors, give glimpses of insight which it required centuries to develop into principles of secure action either as regards the prevention or the treatment of such diseases. On the other hand, the almost entire neglect of sanitary research by Sydenham, confessedly the greatest of English physicians in the 17th century, even when writing at large upon epidemic diseases, shows how few were the principles of preventive medicine that had emerged out of obscurity, even in comparatively modern times; and the long struggle with ignorance and confusion which was required to establish even so simple a case as the dependence of scurvy on an insufficient amount of vegetable juices in the food shows as well as anything else that up to the very beginning of the present century the attention of physicians was but feebly attracted by facts and doctrines which, had sanitary science existed in any real sense, could hardly have failed to become generally accepted.

That it should have taken two centuries to reduce to a practical rule in the navy of Great Britain the measure by which Commodore Lancaster saved his crew from scurvy in 1603 is one of those facts which cannot be got over, and which show that, in a practical sense, preventive medicine and the effective study of the causes of disease is a thing of yesterday as compared with the ages of effort, not wholly unsuccessful, devoted to the cure of diseases. And although the discovery of vaccination alone is enough to redeem the credit of the medical profession during the last hundred years, yet it cannot be said generally that in the great sanitary movement of the present age eminent physicians have always led the van of progress. No one speaking in this place should acknowledge otherwise than with profound gratitude and respect the labours of Edwin Chadwick, of Lord Shaftesbury (then Lord Ashley), and of Lyon Playfair as the pioneers of English sanitary science. Yet it would have been impossible for these distinguished men to have accomplished what they did without the co-operation of medical men, and even of medical authorities. Indeed, the names of Southwood Smith, Neil Arnott, and James Phillips Kay are associated with the movement in this country from its very beginnings in 1842. But the impulse, so far as the general body of physicians was concerned, came from without rather than from within. And it is not to be forgotten also, that M. Pasteur, the most distinguished of living Frenchmen as regards the scientific investigation of the causes and prevention of epidemic diseases, is not a member of the medical profession.

But while it is true that medical science has not always been in a position to initiate discoveries, or to take the leading part in legislation in the direction of sanitary progress, I think it may be claimed for my profession that it has very cordially accepted, and energetically carried out, sanitary reforms of every kind so soon as they came within the scope of practical application by the physician. Of no body of men can it be affirmed with less truth that they have encountered the great reforms of the last forty years in a hostile or narrowly professional spirit. There was, indeed, an evident possibility that it might have been otherwise. To work towards the prevention of disease deliberately and effectually was not only not remunerative, but might have been construed as directly attacking the sources of income. Yet I believe it will be readily admitted that with all but a small and utterly insignificant fraction of a noble profession this consideration has been practically devoid of influence; while the names of John Simon, William Budd, Charles Murchison, Edmund A. Parkes, John Snow, Edwin Lankester, and many others show that even a considerable measure of success as medical practitioners, in the ordinary sense of the word, is in no way inconsistent with eminence and originality of research in the department which has been called in our programme "Sanitary Science and Preventive Medicine."

One name, that of one recently removed from us by death, deserves a separate mention in this connection, in order that we may join in paying a tribute of love and admiration to a character in which eminent and exact science was singularly blended with gentle humanities and modest personal worth known to many of us here—one who, occupying throughout a long career a position subordinate in his official relations, was able through sheer force of genius to influence the cultivation of sanitary science all over the world, by his love of order and method, as well as by the luminous power of exposition he displayed in dealing with what to most men would have been extremely unmanageable and dry arrays of figures. It is not the first time that I have had occasion to express in public my own personal sense of the enormous debt which sanitary and medical science owe to the late Dr. William Farr; and I will venture to quote to you now a few words written in 1861 when, in the course of what I believe was the first separate course of sanitary instruction delivered to medical students in Scotland, I had occasion to expound in some detail the elements of the science of death-rates:—[1]

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[1] *Public Health in Relation to Air and Water*, 1862.

"Dr. William Farr has done for the vital statistics of England almost what Harvey did for physiology or Lavoisier for chemistry. He found the facts of this science in a state of almost hopeless and aimless confusion. He has not only added immensely to the number and value of these facts, but has brought into them light, harmony, order, and, for the first time in the history of the science, a determinate method and an approach to scientific exactness. On the basis of millions of separate details, arranged on the whole in a very close and accurate accordance with medical opinion, and with the demands of sanitary science, it has been the great and enduring merit of Dr. Farr (originally a modest country practitioner of the Company of the Apothecaries) to build up a body of doctrine on vital statistics, not only unequalled, but unapproached in any other country. By the systematic calculation of death-rates he has placed an easy and useful method at the service of all inquirers into the public health,—a method certainly not without risk of error in its application, but giving facilities for the elucidation of truth, and the correction of error, which render it of the utmost value to medical science. By the formation of life-tables from these data, Dr. Farr has also immensely aided the operations of life-insurance; and, by the vast extent of his general information, the vigour of his literary style, and the genial current of his human sympathies, he has been able to invest his dry and abstract inquiries with not a little even of a popular, almost of a poetic, interest. Those who have occupied themselves with the reports of the Registrar-General, or with the most instructive and interesting historical dissertations in the report on the census of 1861, will assuredly join with me in giving to Dr. Farr the credit of having at last solved the problem of investing numbers with a correspondingly real interest, and made blue-books, and big ones too, among the most stimulating and suggestive productions of the age."

It is somewhat painful and discouraging to have to add to these few lines of very moderate eulogy on a now departed benefactor of mankind, that he who thus gave an impulse to preventive medicine and sanitary science amounting almost to the discovery of a *Novum Organum* for it, was in the end obliged to retire from his work on behalf of the nation with so little of what can be called national recognition of his vast labours and eminent services to the State. It is my earnest desire that these words should be taken in a sense void of offence—*i.e.*, as not imputing blame to any one in particular in connection with Dr. Farr's resignation of his office, which was unquestionably made necessary at the time by the state of his health. Nor is this expression of regret in any way inconsistent with the opinion that I have always held and expressed, that the medical profession will not advance its own best interests, or the interests of humanity committed to its care, by entering into the competition for honours, dignities, or emoluments at the hands of those who represent the nation. The claim of Dr. Farr to such recognition was not in respect of his being a medical practitioner, but in respect of his being an officer of State, and one who, in almost every particular of high character and unique distinction, must be considered to have justly earned the very highest rewards that the nation, as such, had to bestow. I hope you will join me in thinking that it is not creditable to us as a people that such services as his cannot, apparently, be more generously rewarded and more highly distinguished during the lifetime of their author. The subject is one, perhaps, not altogether for this Institute; but I have taken the liberty of alluding to it because I happen to know that a proposal to reprint one or more volumes of Dr. Farr's invaluable papers, prepared by himself for such publication, is at this moment in suspense, as it was during his lifetime, because the modest salary of a Government official, and the scanty provision left for his family, did not allow of this being done at the cost, or even at the risk, of those to whom his fame is most dear. Would it not be possible, by a combination of influences emanating in part from this Institute, to secure the co-operation of medical and sanitary inquirers all over the world to this, the best of all possible monuments to Dr. Farr, before the lessons of his life and character have ceased to be vividly present to us? I believe that such a work,[1] could it be provided for either by subscription, or through some society a portion of whose funds might properly be so invested, would become a means of education in sanitary science of the highest possible importance; while a prefatory memoir of the author, carefully and judiciously edited, would leave a moral as well as an intellectual impression which would go far to correct the chilling results of neglect hitherto, and preserve the name of Dr. Farr as an invaluable heritage, not for us only who knew him personally, but for like-minded men everywhere, now and in all coming time. If I earnestly press upon those who are present on this occasion to do what is possible to convert this aspiration of mine into a reality, it is because I believe the future of sanitary science largely depends upon the appreciation of the unselfish labours of those who have devoted themselves to its cultivation in the spirit of William Farr; and I would add, as another great living exemplar, John Simon.

In looking to the future of sanitary science and preventive medicine, it is impossible not to anticipate that it will be as cordially aided by the medical profession in the time to come, as it has been in the past. But just as it has grown in importance and in public appreciation, preventive medicine has also become, not unnaturally, more exacting. It demands a special education, special qualifications, special offices and emoluments, and in these offices the devotion of a man's whole time and energy to preventive and sanitary work alone. I am very far from disputing the propriety and necessity of this arrangement in many cases. But I wish to point out that it carries with it the seeds of an evil influence which it is desirable, as far as may be, to hold in check. The limitation of sanitary functions to men not engaged in medical practice, and of the practice of medicine and surgery to persons who are, or may conceive themselves to be, thereby exempted from all preventive duty, will not be without serious disadvantages. It will tend to split up the medical profession into two sections, perhaps into two more or less hostile camps. It may also tend to make the physician proper, the healer, less careful of the means and appliances of preventive medicine than he would otherwise become. Speaking from my own experience as a teacher, I would say that this would be a grave and perhaps irretrievable disaster to both sides. For the healing art is really, and in a very practical sense, one; and the cure of a disease, if it is to be anything else than mere empiricism, must be largely imbued with a preventive element. In other words, the careful and practical study of the causes of disease with a view to prevention is in many, if not in most, cases a large part of the cure. I have already alluded to the instance of scurvy, where it may be said that the cure emerges directly and exclusively from the knowledge of the prevention. And if it is not so plainly apparent in many other instances that this is so, it is probably only because of our comparative ignorance of the essential conditions both of cure and of prevention. The larger faith now existing in what are commonly called natural means of cure—*i.e.*, physiological as opposed to complicated and disturbing remedies—is the practical fruit of the free recognition of this truth by the medical profession.

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[1] This suggestion has been admirably carried out, under the auspices of the Council of the Sanitary Institute, in a memorial volume of *Vital Statistics*, published in 1885, edited by Noel A. Humphreys, of the Registrar General's Office, Member of the Council of the Statistical Society of London.

Those who are old enough to remember, or who may even now know something about, the complicated and distracting array of medicines and of surgical proceedings which did duty for indispensable remedies in the last age, but which have now passed almost into oblivion, will be in a position to judge how much has been gained for the healing art, in its more limited sense, by the really enlightened modern study of the causes of disease. Were it even possible to suppose that this great advance, in the direction of true science and sound practice, should ever again be undone by the complete separation of preventive medicine from the healing art, the result would, in my opinion, be nothing less than a calamity, both for the one and for the other.

It was a strong sense of the imperative necessity of maintaining this connection between curative and preventive medicine that led, more than anything else, to my acceptance, in 1863, of the office which alone gives me my claim upon your attention to-day. As a teacher of practice of medicine, already of some experience, and recently appointed professor of that subject in the University of Glasgow, I was invited by the authorities to assume the responsibility of the sanitary administration of this great city as its medical officer. It appeared to me that whatever might or might not be the permanence of the connection, the initiation of so great a work was a task not only worthy in itself, but fitted to be of unknown advantage to the students of medicine who might be dependent on me for their instruction. On these grounds chiefly I accepted cheerfully, for what proved to be a term of nearly ten years, the labours and responsibilities of sanitary work. In the discharge of these duties I was aided by a staff of faithful and devoted officers, by the countenance and support of the authorities, and chiefly of that lifelong and still unwearied sanitarian who now sits in the civic chair.[1] It is impossible for me ever to express adequately my sense of the permanent obligation I owe to these men, and to the work in which they were, and are still, engaged. Although the necessities of the city outgrew the original arrangements, and a greatly extended staff appeared to require a different system of administration, I was enabled to retire with a feeling of profound and lasting satisfaction in having acquired a store of literally invaluable experience as a professor of medicine; while, on the other hand, the work has been continued and advanced from year to year by one who has proved himself one of the ablest and most thorough-going sanitary officers in this country—our good friend, Dr. J. B. Russell. I could not venture in his presence to say all that I should desire to say of him, and of the whole sanitary department as at present administered; but I will venture to refer those who wish to know more about it in a compact but comprehensive form, to a paper by him in the *Transactions of the Epidemiological Society of London*, Session 1881-82, new series, vol. i., on "The Policy and Practice of Glasgow in the Management of Epidemic Diseases, with results." I have only time now to revert to some of the more striking incidents of my own decennium of experience.

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[1] John Ure, Esq., at this time Lord Provost of Glasgow, and for nearly a quarter of a century an ardent sanitary reformer, and chairman of the Health Committee of the Local Authority.

During the period referred to—1863-72—we had to deal with several considerable epidemics, which revealed very clearly the weak points of Glasgow sanitation, according to the means and resources which then existed. Among these were a not inconsiderable epidemic of small-pox in 1863; closely following thereon, a really great and disastrous epidemic of typhus fever, lasting from 1863 to 1865, and never completely subdued until it was crossed and merged in a great outburst of relapsing fever in 1870 and 1871; associated with very great want, and misery of every kind, among the poverty-stricken classes who were mostly the victims of this fever. Measles and hooping-cough also prevailed largely during the year 1871; and as a consequence, no doubt, of so much exposure and misery amid such epidemic conditions, inflammatory affections of the lungs and bronchi carried off nearly one fourth of all who died during the year 1871, including, of course, an unusually large proportion of infant lives. Although, therefore, the types of epidemic disease which prevailed in these two years were not nearly so directly fatal as the typhus of the preceding seasons, from 1863 onwards, the death-rate continued high, and it was by no means under encouraging circumstances, except as presently to be stated, that I handed over the medical care of the city to my successor. As these various epidemics had revealed thoroughly our weakest points, there was, on the other hand, room for congratulation on our comparative immunity as regards two diseases which, during my term of office, had given rise to a very large epidemic mortality throughout the country. One of these was cholera, which, as is well known, became epidemic in London during the summer of 1866, and in the autumn and early winter of the same year spread very extensively over England and Scotland, causing an especially large proportion of deaths in Leith, and in some of the smaller towns and villages on the east coast of Scotland. In reporting on this subject on the last day of the year 1866, I was able to show that, notwithstanding our terribly degraded and overcrowded population, still living under the scourge of a previous severe epidemic of typhus, we had lost only one in 16,000 of the inhabitants from cholera in the four months, August to November, 1866, against 7·5 in Edinburgh, 6·2 in Dundee, 7·2 in Aberdeen, 2·5 in Greenock, 5·4 in Perth, and no fewer than 24·4 in Leith. Nor was this because the epidemic influence was wanting; for during the same period more than 700 cases of diarrhœal disease were reported and carefully investigated at the Sanitary Office, 108 of which had more or less resemblance to cholera, and were indeed considered as being in all probability cases of that disease, though often in a mild form. Our comparative immunity was universally attributed—first, to the all-important fact of our having a perfectly pure water supply; and, secondly, to the elaborate arrangements made, under the sanction of the Magistrates' Committee, for dealing with the individual localities and persons affected, in the way of cleansing, disinfecting, isolation, etc. But of these arrangements in detail there is no time to speak at present. They were, however, approved by almost all classes of the community, and brought out in a remarkable manner the active sympathy and Christian co-operation of all the churches, as will appear from the perusal of a special report published on this subject in May, 1867.[1] The other instance in which a very marked check seemed to be imposed by sanitary preventive measures upon an epidemic disease actually widely spread in Scotland at the time, was small-pox in 1871. Not to trouble you with the figures in detail, I will only say that, with one exception (Paisley), Glasgow showed in that year the smallest percentage of deaths from small-pox in all the principal towns of Scotland; and that even with its vast and fluctuating population, open to every kind of epidemic calamity, it has never since been in any considerable degree a prey to that disease. There can scarcely be a doubt, I think, that the attention given to vaccination and re-vaccination from 1863 onwards, as the result of the persistent pressure of the sanitary staff, has been in a considerable degree the cause of that immunity.

In these two instances, then, we were enabled by special circumstances and precautions, the direct result of preventive medicine, to hold our ground in some considerable degree against very serious and appalling epidemics. Why were we not so in the cases already referred to of typhus and relapsing fever? Not, I believe, because these diseases are essentially less amenable to preventive medicine than cholera or small-pox, but because the machinery with which we had to work was wholly inadequate to grapple with their causes. Typhus fever is emphatically, as has been often demonstrated, the epidemic disease proper to over-crowded communities; relapsing fever is the occasional supplement, or additional emphasis, which arises when to a too habitual and extreme over-crowding there is added the presence of something like famine, or at least a grave and general destitution as respects many of the first necessaries of life. In Glasgow we have long suffered, and in the decade referred to we suffered in a very high degree indeed, from both these causes of disease. This fact was so evident on the surface that it could not but attract at once the attention of the medical officers; but their efforts to deal with it in the face of an epidemic actually existing were from the very circumstances of the case foredoomed to comparative failure. In the spring and summer of 1863, I made, along with Mr. Carrick, the city architect, and his assistants, a minute and detailed personal investigation of all the localities in which fever had been reported to us as having taken an epidemic hold; and the results of that investigation were communicated to the authorities in October, 1863, as well as in some earlier and later reports. In many cases, and wherever, indeed, we had reason to believe that improvements could be effected by persuasion, without force of law, we were careful to ask the attendance of the owners or factors of property in the epidemic localities; and in very many, indeed in most, instances we were cordially met, and more or less supported in our suggestions for immediate improvements. Windows not opening at the top, for instance, were dealt with by scores, if not hundreds, and although our instructions were no doubt occasionally evaded, they were rarely entirely disowned. At the same time, disinfection of houses and of clothing was assiduously enforced, one of the first acts of the newly-constituted Sanitary Department being the erection of a special washing-house and other necessary apparatus for this purpose, at which infected clothing and bedding were carefully dealt with in the interest of the community, and free of all expense and trouble to the parties.

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[1] *Report of the Superintendents and Visitors in the Recent Sanitary Visitation Movement, with Considerations as to a Permanent Scheme; also Proceedings of Public Meeting and Appendix of Relative Documents.* Glasgow, May, 1867.

Dr. Russell, in the paper I have already referred to, has told in a connected form the tale both of our difficulties and our partial successes, especially as regards the provision of hospital accommodation for the fever-stricken sick; and how it was a necessary condition, under the first Glasgow Police Act, that a disease should have become frankly epidemic, and been formally so proclaimed by advertisement, before any special powers of dealing with it could be obtained; in other words, that the stable door should be carefully locked only after the escape of the steed had become a very patent fact. But apart altogether from this flaw in our legal machinery, the impression conveyed to our minds, even while thus using to the utmost the powers then existing for the suppression of typhus fever, was (as I often expressed it) that we were merely scratching the surface; and that as long as the great tenement houses —well called "rookeries," and sometimes "warrens"— which abounded in all quarters, but chiefly in the central district, were allowed to exist unreformed, and to be occupied by a class of population constantly deteriorating with the deterioration of the buildings in which they dwelt, there could be no adequate grappling with the problem before us. Two of these "rookeries," as early as July, 1863—one in Argyle Street and the other in the Drygate—had been singled out by us for experiment, in respect of the compulsory diminution of overcrowding, according to the Glasgow Police Act. In one of these I remarked, in first reporting upon it, that "the average size of the rooms (all single apartments) intended for the habitation of a whole family is but little greater than that formally condemned by a Royal Commission as insufficient for a single soldier in barracks." The actual average of all the single apartments in this immense and complicated building was 497 cubic feet, and the actual occupation of them when our operations began was from three to five persons (in one case six persons) in each single apartment. The Glasgow Police Act gave the power in such extreme cases of disallowing altogether the occupation of apartments so small as the above, and thus of putting pressure on the owners to convert single-room houses into "room and kitchens;" and even then power was given to limit the occupation in accordance with a maximum stated in the Act, and indicated in each case by a ticket attached to the door. These powers gradually came into operation, not only in the two large tenements here indicated, but in many others, so that before August, 1864, we were able to report 6360 houses—*i.e.*, individual holdings, mostly single or double apartments—as being under police regulation as "ticketed houses"—*i.e.*, houses the occupation of which was subject to be ascertained by domiciliary visits of the police, and was supposed to be regulated in accordance with the provisions laid down in the Act. In the course of another year (August, 1865) we were able to report a great improvement in the condition of our two first-mentioned "rookeries," which had almost ceased to be fever generators even while the disease was advancing in the city at large. But this temporary immunity of two of the most notorious fever dens, under the extremely stringent regulations adopted, did not carry the conclusion that enough had been done. In the same report that records this immunity, it is affirmed that "without entire reconstruction neither of these properties can be regarded as being brought into a state of proper sanitary regulation." The same remarks might doubtless have been made as regards very many other properties, and especially of a great many in the narrow closes lining the High Street, where a population amounting to nearly 1000 persons on each acre of ground was in constant danger from overcrowding, in many cases in single apartments forming part of large blocks of buildings, with dark airless accesses, and almost every aggravation of nuisance and discomfort—too often with intemperance, immorality, and crime hovering in the track of disease and destitution.

THE GLASGOW IMPROVEMENT ACT.

It was under these circumstances that the late Lord Provost Blackie, in conjunction with the city architect, Mr. John Carrick, and other well-known and philanthropic minds, devised the now famous, and I believe really most effective, City Improvement Act,[1] which was intended to sweep away (but not so suddenly as to give rise to great inconvenience) a large number of those closes and wynds where the worst sanitary features existed, and where, at the same time, there was a prospect that after the clearances a better class of houses might probably take the place of those removed. The administration of this Act has been much canvassed, and even unfavourably criticised in detail; but that it has very greatly diminished the hold of epidemic disease on the parts of Glasgow which it affects, no longer admits, in my opinion, of any reasonable doubt. Now, epidemic disease, from the enlarged and modern sanitarian point of view, is not only a great evil in itself, but also an index to other physical evils of at least equal importance. My attention, as medical officer for the city, was strongly directed to this fact, in March, 1869, when a wave of apparently climatic disease, specially characterized by an enormous increase in the mortality from acute diseases of the chest, swept over the city, and arrested much attention and remark. The Registrar-General (for Scotland) in his reports at the time had ascribed this phenomenon almost exclusively to the "northerly and easterly winds" then prevailing. This and many other popular theories of the matter appearing to me inadequate, I made as careful a research as the available returns then allowed into the whole details of this singular increase in the death-rate, hitherto, there was reason to believe, almost unexampled in Glasgow. I found that while it amounted here to 30 per cent. excess in 1869 (as compared with the corresponding period of 1868); in Greenock the excess was 25 per cent., while in the large towns of the east coast—Edinburgh, Leith, and Dundee—the excess was scarcely more than half as much, and in Aberdeen the difference was quite insignificant. Further, it appeared on more careful analysis that while the excess of mortality was greatest in acute pulmonary diseases, and specially great among the young, the zymotic diseases contributed to it not a little, and also tubercular affections, and among these in a very marked way the nervous diseases of infancy (tubercular meningitis, convulsions, teething, etc.), as contrasted with those of advanced life (apoplexy, paralysis, etc.), which latter showed no tendency to increase. It seemed impossible, therefore, to overlook the evidence that, besides possible climatic causes, there were others operating largely in the case of Glasgow and Greenock to determine so much larger an increase of the death-rate in their case than in that of towns more exposed to cold winds; and that while these could not be excluded from consideration, "the true sources of the excessive liability of Glasgow to such tides of disease and death are to be sought, not in these comparatively accidental circumstances, but in the permanently acting causes of high death-rates, and especially in the low standard of domestic comfort, the overcrowding, general squalor, and physical degradation which are the unhappy characteristics of a large part of the population; and that these again are the direct results of permitting generation after generation to be brought up in houses of the worst construction, in which morality, decency, and cleanliness are alike impossible."[1]

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[1] An interesting account of an early experiment in the same direction, on a small scale comparatively, but guided essentially by the same principles of active beneficence as were at work in promoting this Act of Parliament, will be found in the *Transactions of the Social Science Association for* 1859, by Sir James Watson, whose administration afterwards of the City Improvement Act did much to reconcile it to adverse public opinion.

These conclusions were largely confirmed by further researches made during the years 1870 and 1871, and published in reports presented in the latter year and in 1872. An increase of the death-rate was again subjected to a careful examination, with improved means for its detailed analysis, and at once revealed the large share taken in its production by pulmonary and tubercular diseases, prevailing with increased force wherever the general death-rate was highest and the density of population greatest. Thus it became probable that not only as against zymotic diseases (commonly so called), but also as against those inflammatory, and even those chronic affections of the lungs which form by far the largest factors in the death-rate, the true preventive medicine was to be found chiefly in improvements directed towards the ventilation, cleanliness, and general comfort of the homes of the poor; and that overcrowding and overbuilding of ground space especially were to be corrected, not only as conducing to typhus fever, but to many other diseases. This was the great object aimed at by the City Improvement Act; and while it was not permitted to me to witness, during my official career, its extended operation, I am nevertheless in a position to declare my belief that the policy so initiated was eminently beneficial, and that it is by only steadily following out the same principles that our great cities can gradually uproot the evils of centuries, and become even moderately healthy abodes for those who are attracted into them in search of work. Glasgow is a city peculiarly exposed to numerous causes of ill-health for its population, because (as I expressed the facts in 1871) "upon a relatively smaller acreage than any, excepting Liverpool, it is breeding up, from year to year, a much larger proportion of young lives than any other city. The inference clearly is that the sanitary state of Glasgow is in some way connected with its intense productiveness and its too small superficial area. And any one who is familiar with the high, dark warrens of closely-packed tenement houses in this city; its narrow, ill-ventilated closes; its many miles of such streets and closes, without an open space, without a blade of grass, without a tree or a shrub to be seen; its swarms of children pouring forth from such utterly wretched dwellings through dark passages and narrow lanes, with no playground but the street, no bathing-place but the gutter, can be at no loss to apply the moral of the tale which these numbers unfold."[1]

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[1] *Report on the Health of Glasgow for the first quarter of* 1869, *with special reference to the Unusually High Rate of Mortality in March*, 1869. Presented to the Board of Police by the Medical Officer, May, 1869.

The improvements which have taken place since these words were written are very great, and most creditable to all concerned; but still, as Mr. Chadwick told us through the mouth of the president last night, there is much to do. Let us hope that the authorities of the present and of the future will, as Milton says, " bate no jot of heart or hope," but will go steadily on in the course that has been so well marked out for them.

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[1] Statistical Tables submitted by the Board of Police on Monday, 19th June, 1871; showing the sanitary state of Glasgow, London, and Liverpool, from 1855 to 1870, etc.; with remarks by the Medical Officer. Glasgow, 1871.

IX. — THE PROGRESS OF PATHOLOGICAL SCIENCE; JOHN BAPTIST MORGAGNI AND HIS SUCCESSORS.[1]

THE circumstance of your having unanimously re-elected me as President of this Society at the termination of its first session, thereby imposing upon me the duty and the pleasure of occupying the chair on the present occasion, is one which demands of me not only a grateful recognition of the personal compliment implied in it, but an earnest endeavour to do the best I can to advance the interests and facilitate the business of the Society. I have, therefore, thought that although elaborate addresses form no part of our business, I might, perhaps, with advantage endeavour to become so far your spokesman as to indicate what appears to me to be our true position—the limitations, ends, and methods of our work, and the objects generally that we may reasonably propose to ourselves among the many scientific associations established here and elsewhere for the cultivation of medicine. If I have any personal qualifications for this task, they certainly do not arise from my having been in any sense whatever the founder of this Society; on the contrary, the Society was planned and manned, and its principles and equipment were in great part arranged even in detail, before I was asked to join it. Moreover, I speak my own perfectly frank personal opinion, when I say that my joining you was not in any way essential to your success, nor was it even a material help in the very excellent way you have struck out for yourselves; it is for me simply a legitimate source of honest pride to think that, after so much had been done, the opinion of the early members turned to me as to one able to assist them in organizing their resources, directing their counsels, and presiding over their discussions. I believe that if I can fulfil this duty aright, I shall be doing what I can to advance the progress of scientific medicine in this great community; that I shall be therein helping, as far as in me lies, to lay a solid foundation for the medical art, which at all times needs such a foundation to protect it from the storms and passions of a false and vain popularity; that I shall be aiding the exertions of this University and medical school to train up a body of medical practitioners imbued with the love of nature and of truth; content, nay eager, to know what can be known, but, for that very reason, careful not to pretend to a shallow and showy *omniscience*, either in regard to the causes or the cure of disease. I will go further, and say that, in my opinion, the *educating* influence of such a society as this is its true *raison d'être;* and, were it not that I believe your society is well constituted for such a purpose, my remaining interest in it, at least as a teacher and professor of medicine, would be but small. I shall proceed to justify these statements, and I trust that in doing so I may be able to strike a chord of sympathy with you that will show how far we have agreed hitherto, and how far we are likely to work together for good hereafter. If, as I believe, the Pathological and Clinical Society has its roots in a strongly felt need among you; if that need issues from a desire to build up medical science upon a foundation of solid fact; to give no quarter to vain imaginations, and to have nothing to do with those current phrases too common both among the public and in the profession, by which a half-informed or fashionable sciolism pronounces judgments upon things that are not, and perhaps never can be, known; if, I say, these are the motives that prompted the origination of this Society, then we need not hesitate in believing that it will continue to live and work, even in the far future, when its present members shall have ceased to live and to work;

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[1] An Inaugural Address, delivered as President to the Pathological and Clinical Society of Glasgow. *British Medical Journal*, October 24th, 1874, vol. 2, p. 515.

and that it will be a permanent source of benefit to our science and our art. For the facts to which we appeal are eternal; the nature which we cultivate, even though it be the nature of disease, is only too likely to remain as long as the causes of disease exist in our great communities; and the fluctuating mass of opinion, which we call medical theory, or doctrine, or dogma, whether right or wrong, can no more disturb the basis of fact on which we repose than the winds of heaven can unsettle the great Pyramid. And there is this advantage in a society which professes to be, and, in respect of its plan at least is, independent of such fluctuations; that in all times it may be expected to exercise a healing influence upon all kinds of error and strife, and upon loose and vague assertion in professional discussions elsewhere; not by strongly opposing individual errors, but by presenting to all phases of medical doctrine the unerring and underlying basis of natural fact upon which medical science reposes, and without conformity to which no doctrine or dogma can live more than the life of a day.

We are a Pathological and also a Clinical Society; let us consider the meaning of these two expressions, and consider also how far they differentiate us, so to speak, from other associations having the improvement of the medical art in view. We are, in the first place, a pathological society; that is, our object is the investigation of the doctrine, as based upon the phenomena, of disease. But this object of ours is to be interpreted in the light of one of our fundamental regulations; viz., that communications to this society are to be invariably[1] founded on some visible or tangible object, something which may be judged of by the members independently of the opinion of the writer or speaker; something real, objective, and therefore impersonal, around which discussion is to circulate, and from which it is never, as a rule, to be allowed to stray very far. Here is our radical cure for all false and empty discussion; we sacrifice, it is true, something of mere personal interest, and something also of possible advantage in the airing of various and widely contrasted opinions; but we obtain instead well-grounded statements, and, in most cases, the correction of all serious errors at their source. We desire most fully to acknowledge the advantages which the science of medicine has reaped at all times from the most full and free discussion; and most of us are members also of other societies, where every kind of opinion that is not absolutely trivial or inept may obtain such recognition as it deserves. Our objection, therefore, to theories not founded on obvious facts is, not that we are in any way afraid of them, or even that in all cases we contemn them, but simply that our programme is different; that we give the obvious fact the first place, the theory which springs from it the second; that we prefer to discuss the latter in presence of, and by the light of, the former; that we aim more at the verification of the facts presented to us than at the construction or destruction of the opinions attached to them, although we by no means disregard these when they come naturally within the sphere of our business.

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[1] A departure has taken place, in the later history of the Society, from this first principle in several instances by the interpolation of discussions, under conditions fixed by the Council or by the Society itself, upon subjects specially selected; and these discussions have been very interesting, and have attracted speakers of distinction from London and elsewhere.

The first name of our society, the "Pathological," especially in connection with the explanation now given of its main object, might seem to indicate that we have sprung from the same wants, and are to be guided by the same methods and regulations, as the great and valuable "Pathological Society of London," which has, almost in the very days in which we are now meeting, completed a quarter of a century of its life and issued the twenty-fifth volume of its *Transactions.* The parallel is not undesigned, nor is it without a considerable element of truth; for our objects and aims, though not our actual accomplishments, have in fact a very great and near resemblance to those of the London society. But there is one difference: the London Pathological Society has adopted the expressive motto, *Nec silet Mors,* and has thereby indicated that the chief, if not the only, objects of its special studies are to be the ultimate results of disease as displayed in the dead body; the wrecks, as it were, of our physical being when hopelessly damaged and beyond repair. In these, it has found an abounding and a most fruitful occupation; it has carried out its motto to the full; the dead relics speak, in its *Transactions,* with voices sometimes of warning, sometimes of encouragement, always, or nearly always, of most wholesome and invaluable instruction to the living. It is not because we despise these voices that we have avoided thus limiting our programme, but because, in a smaller field and with fewer resources, we have been naturally guided, as it were, into a different course; and we have, therefore, adopted the name "Clinical," as showing distinctly that *all* the facts and phenomena of disease, in so far as they admit of demonstration, are within the sphere of our operations. And there is in this view of the matter, if I mistake not, one literally immense advantage. It is not for the benefit of science that the living and the dead aspects of disease should be studied apart; nor are they so studied, in fact, either in the London "Pathological" or in its younger neighbour, and necessary supplement, the "Clinical" Society. We hold the processes of disease to be one and the same in kind, whether they issue in the spoiling of a function, or of an organ, or, as most commonly happens, of both together. And we hold, therefore, that the method of observation is the best which conjoins these two fields of observation, and adds to the study of organic ruins that of deranged vital phenomena.

Hence it is that we have assumed the twofold name of the Glasgow Pathological and Clinical Society. We intend thereby to indicate that whatever can be demonstrated in relation to disease, upon the basis of actual facts and cases submitted to observation, is within the programme of business we have set out for ourselves; and, in order to do justice to that business as far as possible, we have arranged to have in the meantime only a limited number of working members, each of whom, however, may introduce a friend, so that our doors are never closed to any one who has facts to communicate, or from whose knowledge of a special subject we may obtain instruction, or who, on his part, may wish to participate in our researches. It is possible that our regulations and bye-laws may in this respect be modified by future experience; but it has been considered best to begin with a somewhat select body of workers, in order that the interest felt by each in the whole body may not flag, and also that mutual confidence may be established, and that the younger and less experienced members may not be overawed or deterred by mere verbal criticism, or by empty discussion for discussion's sake, such as we have sometimes observed to prevail in larger and more miscellaneous associations. Besides, it has been found, as a matter of practical experience, that, among too great numbers, individual cases and facts lose a portion of their interest; microscopic demonstrations, ophthalmoscopic and other observations requiring time, are very imperfectly followed out, and so a habit is acquired of allowing valuable materials to pass without due examination —a habit which would be dangerous, if not fatal, to what I have called the *educating* influence of the society.

Such is our programme, gentlemen, and such the means by which we propose to carry it out. And now let me ask for a moment, What are the benefits which we may expect, individually and collectively, to reap for ourselves and for the medical science and art, from a diligent cultivation of them in the spirit here indicated? Let us look back a little on the course of medical history, and then attempt to realize in some measure what has been and is the tendency of its progress in our own day.

About a hundred years ago there died, at the great age of 90, a man into whose life-long labours we and all other pathological societies have entered, and whose marvellous industry, erudition, and original genius may be said almost to form the link between the spirit of ancient and of modern medicine. This was John Baptist Morgagni, professor successively in Bologna and Padua, and author of the world-renowned and epochal work, *De Sedibus et Causis Morborum, per Anatomen indagatis.*[1] Previously to this great work (and, in some respects, still long after its publication), the conventional and often repeated formal descriptions of diseases and their symptoms still retained almost exactly the pressure of the mould into which they had been cast ages before by Hippocrates and Galen. Even Sydenham (the "English Hippocrates," as he was called), though greatly enriching the description and classification of symptoms from the wealth of his own great personal experience, had not ventured to ask himself, much less to inform others, if apoplexy, and epilepsy, and continued fever, and gout, and dropsy, and phthisis (to take only a few examples), were anything more than in each case a name applied to a group of symptoms, the causes of which were to be found, if found at all, in some fantastic theory of crudity or imperfect action of the humours, rendering necessary a struggle on the part of nature to throw off the unknown force of evil which oppressed her. Such, at least, was Sydenham's view of all fevers, which he regarded as differing from each other only in the different atmospheric conditions which gave rise to them epidemically. Such also was his theory of gout, which he pronounced, after Hippocrates, to be "a provision of nature for depurating the blood of old men;" and, indeed, his ultimate view of all pathology, as contained in the introductory chapter added late in his life to his greatest work, was to the effect that "a disease is nothing else than an effort of nature, struggling by every means to exterminate the morbific matter, and so restore health to the patient."[1] This was, in fact, the old humoral pathology, which held its ground almost unchanged for centuries, during which the science of medicine made scarcely any real progress. And no wonder; for, even admitting that the idea of a *materies morbi* had some elements of truth about it, the use that was made of this idea was so little in accordance with strict investigation, that it became an absolutely plastic hypothesis, fitting in with equal facility to any and every description of symptoms. The theory which ascribed diseases, for example, to a "black bile" (which had never been demonstrated), or to acidity, or an alkaline cause, or a "spontaneous gluten" (as Boerhaave has it), is but one remove from the childish and unscientific conception of occult or direct supernatural agencies, which, even in the fifth century before the Christian era, came under the destructive criticism of Hippocrates in the case of epilepsy —the so-called ἱερὸς νόσος, or sacred disease.[2] All such theories (and the same remarks may be made of the solidist and vitalist theories for the most part, not less than of the humoral) are to be regarded simply as a mode of escape for the mind from the strict control of facts; an "*anticipatio naturæ,*" as Lord Bacon would have said, or rather, a substitution of baseless, or nearly baseless, speculations of the mind for the processes of rigid investigation.

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[1] Published at Venice in 1761.

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[1] *Observ. Med.* cap i. "*De Morbis Acutis in genere.*"

[2] See the treatise *On the Sacred Disease*, 1, 2; compare also the remarks on the effeminacy of the Scythians in the treatise *Of Airs, Waters, and Places*, 22.

Now, to this method of dealing with the facts and symptoms of disease, the whole spirit and method of the great work of Morgagni are in every point opposed. I do not mean to say, of course, that he never falls into such speculations, or adopts them in his reasoning from the current language and philosophy of his time. But, in all essential points, his work is based upon a far more genuine appreciation of real science than was apparently possible to the partisans either of the humoral or of any other so-called philosophy of disease. The method which he opposes to such generalizations is simply that of careful observation and induction, founded upon almost innumerable dissections and histories of disease observed during life. This method (which he borrowed from Valsalva, along with an immense store of facts as the nucleus of his great work) becomes, in the hands of Morgagni, a veritable Ithuriel's spear or touchstone of truth. It is at once destructive and constructive; it shivers false theories to atoms; it replaces them by new and unlooked-for combinations of fact, which of themselves build up a theory, in many cases of permanent and still undiminished value, because it is simply the logical statement of an enlarged and carefully-noted experience. Thus, in investigating the *seats* of diseases, Morgagni is not content to record the *coincidence* of a lesion in an organ with the symptoms apparently due to disordered function in that organ. For the first time almost in the history of medical inquiry, he insists on examining every organ, as well as the one suspected to be chiefly implicated; not only so, he marshals with the utmost care, from his own experience and that of his predecessors, all the instances in which the symptoms have existed apart from the lesion, or the lesion apart from the symptoms; he discusses each of these instances with severe exactness in the interest of truth, and only after an exhaustive investigation will he allow the inference either that the organ referred to is, or is not, the seat of the disease. And, in like manner in dealing with causes:—a group of symptoms *may* be caused by certain organic changes; it may be even probable that it is so; but, according to Morgagni's method, we must first inquire into *all* the lesions of organs that occur in connection with such symptoms; in the second place, we must know if such lesions ever occur or concur without the symptoms; and, again, if such symptoms can be attributed in any cases to other causes in the absence of such lesions. Only after all these questions are discussed in the light of a large and varied experience, will this great master of pathological reasoning admit any absolute conclusion as to the organic cause of the disease.

It would be easy to give ample illustrations of this admirable method from the work of Morgagni himself;

but, as the object of this address is not historical nor literary, but practical so far as we ourselves and our work here are concerned, I prefer to look at what Morgagni's pathological and clinical method has done for us in the light not only of his own researches, but of those of his successors. For, from this point of view, all the more eminent moderns, and even the men of our own time, although they work with new means and appliances, amid a flood of new light from physiology and histological anatomy, and amid a science of organic chemistry which it may be said has been created, and extended, and applied, within the present century almost exclusively, are all of them successors and legitimate heirs of Morgagni's labours and method, in so far as they aim at uniting clinical with pathological research by strict processes of reasoning founded on multiplied observations. For it is this method and this spirit that make the essential distinction of the modern-minded physician or surgeon, and that separate him *toto cœlo* from the man whom Molière has depicted for us in caricature—most laughable, and, perhaps, unfair, but still not quite outrageous caricature, if we may trust contemporary letters and documents. The men of the century preceding Morgagni were divided into hostile camps; and it may be said that their discussions, so far as we know them, were generally not about facts, but about mental abstractions or scholastic wire-drawn analogies. Theories of diseased action so remote from evidence, and so comprehensive that it was impossible to get beyond the sweep of their drag-net, occupied all the schools, and gave the tone to the teaching from every professor's chair. "Under which king, Bezonian?" was the question addressed to every pupil and almost to every practitioner, who was almost compelled to be a humoralist, or a solidist, or a vitalist, without exactly knowing why he was so. And the practice was like the pathology; it was either modelled slavishly on custom and tradition, or it broke violently loose from these upon the basis of some outrageous dogma, which did not even assert for itself a birth out of carefully-studied evidence, but only out of the dust of the arena, the fierce polemics of contending factions. It was scarcely a very violent caricature of the practice of his day, when Boileau compressed its results as regards the patient into one stinging line—

"L'un meurt vuide de sang, l'autre plein de séné,"

or even when Molière, out of his unbounded contempt for the whole apparent resources of the art, represents the whole chorus of the Faculty as loudly approving the barbarous statement of them by Argan—

"Clysterium donare,
Postea seignare,
Ensuita purgare,
Repurgare, reseignare, et reclysterizare."

I do not mean to assert that Morgagni himself entirely escaped the influence of these debased traditions of art, or that we his successors have in all cases even now got clear of them; but it may be safely said that the foundations were laid by the Professor of Padua for our escape, and that all the good work done and yet to do, in this direction, has been, and must be, guided by the spirit and method of his work. And in this sense I claim not only the professed and exclusive morbid anatomists, but also, and still more, almost all the greatest physicians and surgeons of our own and the last century, as the legitimate successors of Morgagni and the inheritors of his method of working. Without him, we should probably have waited much longer for Laennec, and might very probably have been at this hour without the stethoscope, and all that it has brought us. Without him, we should almost certainly have been without those means and appliances in all our hospitals, which produced for us in this country Baillie, the Hunters, Astley Cooper, Richard Bright, Abercrombie, Hope, Watson, Latham, and a host of others both at home and abroad, who have contributed their stores of experience to an investigation of disease at once clinical and pathological. The pathology of continued fever and the differentiation, upon sure grounds of fact, of its varieties; all the work, in fact, which is represented by the labours of Bretonneau and Louis in France, of Stewart, Jenner, and Murchison in this country, is a direct emanation from the school and method of Morgagni, and has, like the work of the Italian physician, been quietly dispersing a whole dark cloud of visionary pathology and erroneous practice; the old names of febris pituitosa, biliosa, nervosa, putrida, maligna, etc., have disappeared, and order has entered in where there was formerly a chaos; as a consequence of this, our information as to causes has grown to a reasonable degree of precision, and preventive medicine has obtained access to a realm where the blind fear of contagion was at one time the sole dominating idea. And, although we have as yet no grand curative discovery to boast of in this department, yet who shall say that it is nothing to have studied accurately the natural history of these diseases, and so to have been saved from the errors of Broussaisism on the one side, and Brunonianism on the other? nothing, that the whole realm of the continued fevers of this country has been rescued from confusion; that diagnosis has been rendered exact, and statistical conclusions possible? For it is strictly in accordance with historical truth to say, as I now do say, that these results we owe to a rigid application, by many and varied minds, of principles derived in great part from the work of Morgagni.

Then cast a glance, if only but for a moment, over the great results which the same clinical and pathological method has worked out for us in the departments of cardiac, renal, cerebral pathology; how, in the memory of men now living, the diagnosis of valvular diseases, embolism, uræmic poisoning, and a host of collateral conditions formerly entirely obscure or misunderstood, has been brought into daylight, and connected securely with the history of the corresponding organic lesions. Think what an advance has been made since the days when delirium tremens was supposed to be a kind of phrenitis, and treated with indiscriminate bleeding; when *apoplexia serosa,* now known to be almost always a sequela of serious, and for the most part irreparable, organic changes in the kidney, was regarded as evidence of congestion of the brain; when valvular disease of the heart was almost sure to pass for asthma, or dropsy, or hæmoptysis, and to be treated without reference to its cause in the central organ of the circulation; when embolism, white softening, calcareous and other degenerations of the arteries, etc., were unknown as the causes of apoplexy or paralysis, and almost all cases of sudden disorder within the brain were regarded as congestive or inflammatory, and treated by bleeding or purging; when attacks of mania, even occurring in connection with general paralysis or in the dementia of epilepsy, had no other pathology than an inflammation or congestion supposed, and only supposed, to exist; when the delirium of fever had most frequently a like explanation, and was too often met by the inevitable emission of blood. The changes of our point of view in these and many other similar instances we owe to an exact and detailed study of the visible and tangible acts of disease, pursued essentially after the pathological and clinical method initiated by Morgagni; and in every instance the result has been the dispersion of a host of vain imaginations, which had led the minds of men for centuries, and contributed largely to a most irrational practice.

But modern pathology has not contented itself with these triumphs. Its aim now is not only to grapple with the question of the "seats and causes of diseases," in the sense in which Morgagni understood the expression, but to investigate and unfold the more intimate nature of pathological processes; to trace the intricate changes in the tissue-cells which constitute inflammation, tubercle and cancer; to make visible through the microscope, and sensible through a refined chemical analysis, the degenerative processes through which heart, and liver, and kidneys, and arteries, and brain are rendered unfit for the performance of their functions; and here again, to trace out by a more exact and a much more refined examination of symptoms, the relation of the functional disturbance, as observed during life, to the essential organic change. It seems, indeed, as if we were going back again to the old and fruitless research into the proximate causes of disease; but with this difference, that what we seek now is not a mere notion embodied in a word, such as were the alleged causes which occupied so much space in the ancient pathologies, but a positive result of science resting upon detailed evidence, and only open to controversy in so far as exact observation, and carefully performed experiments are liable to vary and to fail even in the most practised hands. But to carry this new science of pathology to its utmost limits, nay, even to appreciate its processes and verify their results, requires no less than that the observer should have spent, and be willing to spend, hours upon hours continuously over his microscope, or in the chemical laboratory, or in the extremely delicate and absorbing work of physiological and pathological experiment. The *Cellular Pathology* of Virchow, with all the detailed observations and doubtful questions that have arisen out of it; the exquisite researches of Cohnheim and Recklinghausen; the experimental, physical, and physiological results obtained by Klein and Sanderson, and many other and younger workers, seem to be leading the way to new depths of science that can only be sounded through the absolute devotion of a lifetime to new methods of research. It is here, I confess, that I begin to feel as if my proper place were not exactly to be at the head of this society. Having served my apprenticeship to some of these advanced methods, in days now long gone by, I am free to confess that the more recent advances have carried the more strictly scientific studies in pathology into regions far beyond my depth. But I have one consolation. If I am too old to teach, or to investigate with success in these novel lines of investigation, I am not too old to learn their results from you. In your excellent Vice-President, Dr. Joseph Coats, you have a man admirably qualified by study and capacity, as well as by personal experience, to keep us all from going astray in these untrodden paths. And for myself, though I may fail in these most recondite departments, I may possibly still be allowed to present to you, from time to time, the results of clinical experience, and so to help you to work out your programme of a Clinical, as well as a Pathological Society.

[In resuming the office of President, October, 1888, by an unanimous vote of the Society, Dr. Gairdner gave a short address supplementary to the one here printed : and said that during the fifteen years of the Society's existence, it had very fully carried out the programme of work which had been adopted in the first instance. In further illustration of the practical value of the method here adverted to, he indicated the recent advances in cerebral physiology and pathology, and the splendid surgical results recently laid before the British Medical Association by Dr. Wm. Macewen (a Member of the Pathological and Clinical Society) as the late fruit of a long course of clinical, pathological, and experimental research, pervaded throughout by the spirit and method of Morgagni.]

X.—HOMŒOPATHY.[1]

"OBSERVATION, meditation, and experience," writes Hahnemann in the introduction to his chief work, the *Organon of the Healing Art*, "have discovered to me that, according to the precepts of homœopathy, the course to be followed in order to obtain gentle, swift, sure, and durable cures consists in choosing, in each case of disease, a remedy capable in itself of producing an affection similar to that which it is desired to cure. This homœopathic method had not been taught by any one before me; no one had put it in practice. But if it alone be consistent with truth (as every one may be convinced as well as I), we may expect that, notwithstanding its long concealment, every age will, nevertheless, offer palpable traces of it. This has, in fact, been the case. In all times, all diseases which have been cured really, promptly, durably, evidently, by medicines, and which have not owed their cure to the accidental concurrence of some favourable circumstance such as the natural termination of an acute disease, or the gradually reinforced bodily powers obtaining a mastery over the disease during an allopathic or antipathic treatment (for a direct and an indirect cure are very different),—all such diseases, I say, have yielded, although the physician did not know it, to a homœopathic remedy, that is to say, to a remedy having the power of exciting of itself a disease resembling that of which it caused the removal."

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[1] *Edinburgh Essays, by Members of the University*, 1856. Adam and Charles Black, 1857.

In another passage, Hahnemann thus briefly expounds the results which he ascribes to the homœopathic method. "When the application of a medicine, chosen so as to be perfectly homœopathic, has been well made, the natural acute disease which it is intended to cure, however malignant and painful it may be, is dissipated in a few hours if it be recent, and in a few days if it be of somewhat longer standing. . . . Chronic diseases, and especially those which are complicated, demand more time. Those chronic medicinal diseases especially, which allopathic medicine engenders in addition to the natural disease which it has been unable to cure, demand a very long treatment, and are frequently incurable."[1]

In the above extracts we strike, as it were, the keynote of the passage in the history of medicine which it is proposed to examine in this article. We are enabled, in particular, to appreciate two of the characters by which it is distinguished. Homœopathy is a *universal* and an *exclusive* system. It professes to meet the case of every curable disease; and to be the only method by which diseases are really curable. It assumes, in other words, to have reduced the entire cure of disease by medicines to a single therapeutical law; and it puts forward that law as a new discovery. In submitting ourselves, therefore, to the guidance of homœopathy, we cannot avoid rejecting all other medical doctrines, in so far as they relate to the action of remedies. We are led to the conclusion that medical truth is a thing of yesterday; that up to the promulgation of the "homœopathic law," the cure of diseases was the result of accident, or of a blindly-working and fluctuating experience; but that it is now based on principles as secure as they are simple, and requiring for their application only an exact knowledge of the properties of drugs on the one hand, and the symptoms of diseases on the other.

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[1] *Organon,* § 149.

Hahnemann did not shrink from maintaining the total and irreconcilable antagonism of his own system of doctrine to all those current in his own day. In his classification and nomenclature of therapeutical methods, Homœopathy, Antipathy, and Allopathy appear to exhaust the possible applications of remedies to the cure of disease. The first of these terms, which he derives from the Greek words ὅμοιος, *like*, and πάθος, *a morbid state*, he applies to his own system, the formula of which is contained in the words "*similia similibus curentur,*" *let like things be treated by like.* To the second term, from ἀντί, *opposed to,* and πάθος, he assigns an exactly opposite formula, viz., "*contraria contrariis curentur;*" and of this mode of practice he professes to find numerous examples in the medicine of the past. All medical practice and all therapeutical doctrine not coming under either of these formulæ is, according to Hahnemann, only to be regarded as a blind attempt to discover the proximate or hidden causes of disease; remedies being then applied according to their presumed influence upon these causes, and not from any relation of their effects to the obvious symptoms. On this ground, Hahnemann termed the great mass of medical practice Allopathy, from ἄλλος, *other,* and πάθος, and he finds constant employment for his sarcastic pen in contrasting its complexity and confusion with the simplicity of the homœopathic system, which, according to him, stands alone and apart, refusing to ally itself with other methods, or even to accept from them that temporary aid which, in some instances, they appear to be capable of bestowing. He repeatedly asserts, that allopathy and antipathy, while occasionally purchasing a very slight and temporary relief from suffering by subsequent aggravation of the disease, are responsible for a large proportion of the sickness and mortality from chronic diseases; of which a great number, and these the least remediable, spring solely from the use of active drugs by physicians and apothecaries. The homœopathic law, on the other hand, is, according to Hahnemann, of universal application in theory, and is limited in practice only by the limited number of remedies, and by our imperfect knowledge of their action upon the human body. It claims as its own every real cure; it excludes, within the limits of its application, every possibility of failure. It condemns with equal severity the dissentients from its own exclusiveness, whether they range themselves among its avowed opponents, or attempt to bring about a compromise of opposing theories. "It is as impossible," says Hahnemann, "to conceive of another true method of curing dynamic diseases, as it is to draw more than one straight line between two points."[1] As to the practitioners who pursue a mongrel system, and, by calling in the resources of allopathy or antipathy, acknowledge the insufficiency of the homœopathic law, there is no excuse for them. They are no true homœopathists, and are, if possible, worse than the partisans of the "hateful methods" to which it is so diametrically opposed. "The pure homœopathist, who almost never misses his mark, who succeeds almost always, will never consent to an association of this nature."[2] The homœopathic law, therefore, is, according to Hahnemann, the single "Organon" of medicine. It is the exclusive and universal method of cure. It is a system "of great simplicity, always the same in its principles and in its practice, forming a separate whole, perfectly independent of other systems, and refusing to associate itself with the pernicious routine of the old school."[1]

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[1] *Organon*, § 109, note.

[2] *Organon*, § 109, note.

I have been thus particular in insisting on the universality and exclusiveness of the so-called homœopathic law, because it is evident that a system so characterized cannot be tried by the ordinary canons of criticism. Not only is the great experience of the world against it, *primâ facie*, but the assemblage of instances in its favour will prove nothing, unless the opposing instances are satisfactorily explained away. When it is asserted that bleeding is useful in certain inflammations, or opium in certain diseases of the nervous system, it is not necessary to demonstrate that over the whole range of these diseases, still less in each individual case, the fact asserted holds good. It is enough if we succceed in showing that, in some cases of inflammation or of nervous disease, bleeding and opium respectively are used with good effect. Very different is the position of the homœopathist. He has bound himself over to establish, by sufficient proof, not a more or less particular fact, but a general law of nature, including and superseding all others within the sphere of its operation. He has himself chosen this issue, and must abide by it. Had Hahnemann asserted only that *certain* remedies resemble, in their operation upon the healthy body, the diseases they are employed to cure, he would have but asserted a truism; for every one knows, and no one disputes, the action of ipecacuanha in some kinds of sickness, and of rhubarb in some kinds of diarrhœa. It is the assumption that we can generalize this fact into a universal principle or law that constitutes the peculiarity of the homœopathic system, and at the same time gives rise to the difficulty of dealing with it by means of instances.

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[1] Preface to the *Organon*.

The contumely which Hahnemann's pugnacious nature and lax style of declamation led him to pour out so freely upon the systems which he ascribed to his opponents, increases not a little the difficulty of submitting his works to a calm and serious examination. There are few medical writings in any age so careless of logical method, and so fruitful of bitter invective as his; and, even in the ecclesiastical arena, it is rare to find a disputant so ready to foreclose argument by strong assertions; so little anxious to convince, so forward to strike and to wound. The thunderbolt of an assumed infallibility is never out of his hands. In perusing his writings, we seem to see the opponents of the new theory pursued by an avenging Nemesis, from which there is to be no appeal. The experience of ages is no defence, but an aggravation of their crime; the multitude of witnesses on behalf of the "ancient medicine" does not entitle it to a more respectful consideration, but rather calls more loudly for that summary vengeance which their great conspiracy against the truth is sure to call down on the head of "allopathic" physicians. It is difficult, it must be confessed, to preserve one's equanimity in the midst of such a highly-charged electrical atmosphere as that of the Hahnemannian polemics; but we shall do our best.

> "Could great men thunder
> As Jove himself does, Jove would ne'er be quiet,
> For every pelting, petty officer
> Would use his heaven for thunder; nothing but thunder!"

The following passage may serve as an example of the "*saeva indignatio*" here alluded to. It occurs in the preface to Hahnemann's chief work, the *Organon der Heilkunst.*

"The end of the fatal efforts of the ancient medicine would appear to be not less than this,—to render incurable, perhaps even mortal, ninety-nine hundredths of the diseases which assume the chronic form; and this end is attained without difficulty when the practitioner is fully acquainted with the accredited methods of treatment, and has become deaf to the voice of conscience. He then either weakens or torments unceasingly the feeble invalid, already overwhelmed by his own misfortunes, or he brings upon him new and terrible diseases. Arguments are not wanting to the allopathist to defend all the mischief he does; but he never seeks support save in the prejudices of his masters and in the authority of his books. . . It is not until (being convinced by long practice of the dreadful effects of his pretended art) he limits himself to insignificant drinks (that is to say, to doing nothing even in the gravest cases) that his patients begin to be less frequently rendered worse, and cut off in his hands. This fatal art has, for a long series of ages, wielded arbitrary power over the life and death of the sick; has destroyed ten times as many men as the most murderous wars, and enormously increased the sufferings of many millions of others."

One is frequently reminded, in reading these and other similar passages, of the sledge-hammer style of eloquence which procured for Paracelsus among his opponents the title of "Lutherus Medicorum." Hahnemann was a ready penman, and an able, though unscrupulous, controversialist; and there can be no doubt that, by his virulent opposition, extravagant as it was, the overweening pretensions and unstable theories of medicine which have been ever too much in favour, and were current to an unusual degree at the time he wrote, received a check which was needed, and which has, on the whole, been useful to the medical art. Nor is there wanting in some of these tirades, even amid the predominating bitterness of spirit, a certain grim humour which entitles Hahnemann to a place among the satirists. In a literary point of view, however, they do not rank very high among the numerous assaults on medical pedantry and absurdity. The accurate and profound erudition of Rabelais, the delightful frankness and amiably egotistic philosophy of Montaigne, the thoroughly laughable and effectively broad caricature of Molière, are wanting to point the ridicule of Hahnemann, and to temper his vehemence. Instead of these, we have ponderous displays of ill-digested reading, guided by no critical faculty, and tending to reduce all testimony to a dead level; systematically distorted reflections of medical science; with an amount of arrogant dogmatism, and self-confident assertion, to which it is very difficult to accord the credit of sincerity. We are never allowed to forget that we are in the presence of one who claims to be the possessor of exclusive truth, and with whom the sacrifice of a hecatomb of reputations is but the logical consequence of an inexorable system—the necessary means to the exaltation of the "new medicine, which *alone is true.*" We admire the strength of will—we smile at the boastful pretensions—we indulge a momentary feeling of indignation at the sweeping and virulent invective; but we find little in these boisterous outbreaks of medical fanaticism to satisfy either the reason or the moral sense. The special pleading of an advocate is manifest throughout; and of an advocate compelled, by want of materials for a calm and reasonable appeal, to resort to indiscriminate abuse. The line of argument (so far as such a thing can be traced in these torrents of invective) is as follows:—The causes of all diseases, except surgical or mechanical diseases, are hidden; and hence allopathy (that imaginary system which Hahnemann everywhere sets up as a target for his sharpshooting) is nothing but a system of complicated absurdity, in which remedies, themselves productive of disease, are given in a cumulative manner; so that the original symptoms are lost in the crowd of artificial maladies, and the last state of the patient is worse than the first. Thus, opium may be selected to procure sleep, and may succeed in accomplishing this single object; but as opium is known to act upon the skin, the kidneys, the intestinal canal, as well as upon the nervous system, other drugs are associated with it for the purpose of preventing these collateral effects; these drugs are productive of a host of disorders, which, in their turn, require renewed interference, and in this manner are engendered all those complicated, functional, and organic affections which usually mark the course of the most severe and fatal diseases. Allopathy, by its blind routine, by its large doses of active drugs, and by its complex prescriptions, is therefore not only ineffectual as a curative system, but becomes the fruitful parent of disease. In all these respects it is opposed to homœopathy, which, besides a curative power unknown to any other system, can lay claim to the virtues of perfect simplicity and perfect safety. We shall have occasion to consider some of these assertions in the sequel.

Having thus endeavoured to define the position which homœopathy claims for itself in relation to medical systems and medical practice in general, I shall proceed to consider the evidence by which its claims are supported. We may divide this subject conveniently into two portions: *first,* the homœopathic law in the abstract, or as inferred from selected instances; *secondly,* the homœopathic practice as bearing on the truth of the alleged universal law on which it is founded. It is evident that these two inquiries are not only capable of being distinctly pursued, but that they will produce confusion if mingled together; for it is possible, on the one hand, that the law may be well founded, although its general application in practice be unattainable; or, on the other, that the law may have been inferred on insufficient evidence, although the practice be on the whole good, *i.e.,* an improvement on that ordinarily pursued.

I. *Examination of the Homœopathic Law.*—Let us now see how Hahnemann builds up the doctrine which is, in fact, the foundation of his system. We are justified in looking to Hahnemann's writings as the most authoritative exposition of this doctrine, because neither he nor any of his followers professes to find a distinct expression of it in previous medical history; while, on the contrary, as we have seen, they put it forward as a direct contradiction to all antecedent generalizations.

Nor am I aware of any attempt on the part of the homœopathists to revise the process of reasoning by which this doctrine is alleged to be established on the ruins of the past. On the contrary, the latest, and unquestionably the ablest and most distinguished of the homœopathic controversialists, has in great detail, and without one word of unfavourable commentary, narrated the whole process of this investigation according to the account given by Hahnemann himself. "The idea of the homœopathic law was reasoned out," says Dr. Henderson, "*before a single testing experiment was made.* The first experiment was made with cinchona bark."[1] The narrative to which this remark applies is to be found in the letter of Hahnemann to Hufeland, quoted by Dr. Henderson. But this was one of the earliest publications in which his ideas were expressed otherwise than in a fragmentary form. My observations will, therefore, for the most part, be founded on the *Organon,* the author's most important and most matured work.

I propose first to examine the logical process by which the law of homœopathy was "reasoned out before a single testing experiment was made." This abstract conception of the law occupies the first place in the history of its discovery. The value attached to it by Hahnemann, however, was not merely historical, as we find it occupying the first place even in the

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[1] *Homœopathy fairly represented;* by William Henderson, M.D.

Organon. Hahnemann's first thesis, therefore, with respect to the homœopathic law professes to be a logical deduction from the nature of disease and the nature of remedies in general. Let us see how he establishes his position.

All disease, according to Hahnemann, is a modification of the vital force (which in health determines the functional activity of the organism) by external influences of a deleterious or morbific kind. But as the deranged vital force is at the root of all disease, and as the vital force is an immaterial force (" a spiritual dynamis," according to Dr. Dudgeon's translation), it follows that morbific influences can only act in an immaterial (or " spiritual ") manner on the vital force, and likewise that " curative agents are able to re-establish, and do actually re-establish, health and harmony, only by their immaterial (spiritual or dynamic) action on the vital force." Now, as everything immaterial is of necessity beyond the reach of the senses, it follows that the original disturbance of the vital force, as well as the essence of its cause and of its cure, must always be hidden from our direct observation. But the Author of Nature has so arranged that the hidden modifications of the vital force in disease find their perfect exposition in the form of symptoms, *i.e.*, morbid phenomena accessible to the senses. To our senses, therefore, *the symptoms are the entire disease,* and each separate alteration of the vital force is manifested in a special series of symptoms corresponding in character with the nature of the morbific influence. It is of no avail, according to Hahnemann, to inquire how the altered vital force produces diseases. Still less philosophic is it to ascribe to disease a separate material existence within the organism, as is often done by medical theorists (allopathists). "The Master of life has rendered accessible to the senses only those phenomena of disease, the knowledge of which is necessary and sufficient for the cure; and the cure accordingly consists solely in the removal or annihilation of the symptoms, which, when taken as a whole, represent accurately the whole of the disease. All speculation beyond this is futile and absurd."—*Organon*, §§ 6-18.

Observe, before going further, that to this general exposition of doctrine Hahnemann incidentally admits two classes of exceptions, which may, however, perhaps be considered as the same. The one is that of surgical or mechanical diseases (§ 13), in which the essence of the disease may be accessible to the senses, and therefore amenable to mechanical processes of cure; the other, where a *causa occasionalis* or manifest exciting cause of disease (§ 7) exists, and is capable of being removed. Of these exceptions there is more to be said hereafter.

To pursue Hahnemann's reasoning. As the only knowledge we have of disease is through symptoms, and the only knowledge we have of the effect of remedies is through their effect on these symptoms (the curative or dynamic essence being, like the vital force on which it acts, immaterial and hidden), it follows that remedies can only act on diseases in virtue of their power to alter the vital force, *i.e.*, " to produce (in healthy persons) certain accidents or symptoms, or, to express it more clearly, a certain artificial disease which destroys the natural disease already existing." It follows also that, to annihilate the totality of the symptoms of a malady, we must seek a medicine which has the power of producing either *similar* symptoms, or *contrary* symptoms, to those of the disease, according as experience teaches us to use the one or the other. Experience, however, has already pronounced a verdict upon this issue by showing, as the result of all careful trials, that morbid symptoms can only be temporarily palliated, while they are in reality aggravated by remedies administered on the antipathic system. The allopathic system, again, is a mere imitation of the blind efforts of the vital force to get rid of a noxious influence by creating, at all costs, a disturbance in the organism; it can never do good except by chance. The homœopathic method, then, of adapting remedies to symptoms is the only one which remains. Therefore, *per viam exclusionis,* it is the only true system.—*Organon,* §§ 19-24.

Should the reader be astonished at this strange specimen of medical logic, I beg to assure him, nevertheless, that, to the best of my understanding, I have faithfully reported it. In doing so, it has been necessary to reduce a great deal of repetition and *verbiage* within a moderate compass; but I am not aware of having suppressed or understated a single point in the original argument. In the letter to Hufeland, most of the same reasoning reappears in a less abstract form. The ultimate appeal, as in the above passages, is to the alleged failure of the ordinary practice (allopathic and antipathic). This failure being assumed, it is "blasphemous" to conceive that the medical art is incapable of greater certainty; "that the infinite wisdom of the eternal Spirit that animates the universe could not produce remedies to allay the sufferings of the diseases He allows to arise;" or that God "was capable of the tyranny" of leaving the cure of any disease an insoluble problem. "There must be a sure and trustworthy method of treatment as certainly as God is the wisest and best of beings;" for it would be "indifference to the tortures of His best beloved creatures" to have denied to the genius of man the discovery of positive and certain remedies, with their "easy, sure, and trustworthy" application to the cure of disease. The arcanum thus yielded, after centuries of darkness, by the divine beneficence, is, of course, Homœopathy. I confess to a degree of reluctance in quoting even so little as is here given of these fanatical, and, as it seems to me, almost impious reflections upon the divine government of the world. They afford a striking illustration of the license allowed to itself by an ill-regulated intellect, acting in a self-willed and arrogant nature. We have already seen how Hahnemann dealt with human science and learning; in these passages we see the same scornful and vehement spirit attempting, as it were, to scale the heavens—to lay down laws for the Creator of the universe, and to pluck from His grasp, under the threat of an accusation of injustice, thinly disguised in reverential language, the elixir of life for which the alchemists laboured in vain. We are shocked by the thought that the "tyranny" and "indifference" against the imputation of which Hahnemann seems, in the above sentences, to defend the Deity, have actually been displayed towards mankind for several thousand years, during which they have, according to Hahnemann, lived in a state of medical darkness. It is painful to find these fanatical aberrations quoted approvingly by homœopathic writers as evidence of a "lofty piety," and as "the solitary instance" of arduous medical researches undertaken on the special ground of a deep sense of the divine goodness.

With regard to the argument, or rather the hypothetical basis, on which the homœopathic law is made to rest in this portion of the *Organon*, I have only a few remarks to make.

First, This argument assumes as proved a theory of disease which never can be proved, and which can, at most, be only partially true, inasmuch as it is clearly untrue of some diseases, and probably not entirely true of any. That in all diseases, whether of function or structure, there is some modification of the forces of life, or the conditions under which these forces act, is true; but this is simply a definition of disease, not an inquiry into its nature with a view to its cure. Disease is disordered function; disorderedfunction implies a modification in the action of those forces which preside over function in the living being—a modification, in other words, of the vital activity, or vital force. But in the raising this vital force to the rank of a metaphysical entity; in endowing it with powers of acting and suffering distinct (in theory) from those of the bodily organization in which and through which it acts and suffers; in assigning to it, in short, a spiritual or immaterial essence, from which disease *directly* springs, and on which remedies *directly* act, Hahnemann has transcended the sphere of science, leaving behind him the whole of the facts which it was his duty to investigate, and if possible to make the basis of his theory of disease. He has, in fact, committed the same error as Van Helmont and his predecessor Paracelsus, when they ascribed the whole phenomena of health and disease to an Archæus; who, from his unseen throne in the centre of the Microcosm, gave and administered laws, suffered and struggled, was oppressed or victorious, angry or pleased, restless or at peace. The results of this fundamental error are plainly visible in every step of the subsequent reasoning. To connect symptoms with the immaterial essence of disease, Hahnemann has to call in the aid of a special arrangement of divine providence, by which, as the keys of an instrument are made to correspond with the chords (*i.e.*, artificially and for a specific purpose), the symptoms are placed in relation with the states of the vital force, only that we may be informed of so much as is necessary to the cure of disease, which is in itself an inscrutable and intangible essence. In like manner, in order to connect the curative virtues of material remedies with the aberrations of an immaterial vital force, Hahnemann is compelled to avail himself of another assumption, not less violent than the former,—that remedial substances, known as they are to act upon the healthy organism, are endowed with their physiological properties simply as an index to their therapeutic virtues, which are not capable of being the subject of direct physical demonstration, but are occult, dynamic, immaterial, specifically adapted to the specific changes in the immaterial vital force. In criticising the ordinary imperfect theories of disease as not justified by our knowledge of its phenomena, Hahnemann may not be very far wrong; but he only escapes the error which he ascribes to allopathy by plunging into a sea of abstract speculations, founded on assumptions without proof, having no actual or possible place in demonstrative science; which it is, therefore, impossible to confirm, and equally impossible, as well as useless, to confute.

Secondly, The argument of Hahnemann breaks down at every point when applied to certain well-understood forms of disease, some of which he has himself indicated. Thus, mechanical diseases requiring surgical appliances are excepted from its operation. But why? Is not a stone in the bladder a most palpable disease, and is it not removed by a most palpable remedy? Do we not touch here, by means of knife and forceps, that very proximate or essential cause of disease, *quæ præsens morbum facit, sublata tollit?* And do we not, having this proximate cause or essence in our view and in our power, practically determine to leave the mysterious vital force, with all its occult operations, to shift for itself, while we lay hold of the mechanical and tangible obstruction, and by removing it, cure the patient? Now, suppose the obstruction to be of a different kind—a tapeworm in the intestine, an acarus in the skin, a gallstone in the biliary duct, an undue fermentation of the food, an indigestible mass in the stomach, a poison in the blood. Of most of these sources of disease, considered as essential causes of certain symptoms, we have incontrovertible proof; some of these causes we can remove by medicine, thereby removing the disease; some of them we cannot so remove. But with most physicians the same *principles* apply to all, *i.e.*, removal of the cause, if that be possible—palliation of the symptoms, if not. Hahnemann, on the contrary, divides these precisely similar cases into two, or perhaps into three classes. Where the cause (*causa occasionalis*) can be directly removed, he will not absolutely shut his eyes to the necessity of this proceeding; but where it cannot, or where the nature of the cause is only a shade less palpable than a stone in the bladder, *i.e.*, where the disease is internal, and without an evident *causa occasionalis*, he will refuse to seek it further, and will take refuge in the mist of an immaterial essence, only to be reached by specific remedies. Palliative remedies and empirical attempts at cure he equally repudiates. Disease *must*, above all, be *certainly* and *easily* curable; if possible, by an intelligible theory, if not, then by a theory founded on gratuitous assumption, and clothed in the panoply of a scholastic logic worthy of Thomas Aquinas. Yet who shall say that we are wrong, if we assert, in opposition to Hahnemann, that the great mass of the ills that flesh is heir to occupy precisely this middle position, which Hahnemann will not admit? Their cause is, to a certain extent, removed from observation, and perhaps still further removed from the action of remedies; or it is complex, and is only in part within our reach; but it is still a material cause, as cognizable in theory, possibly as amenable to practice (if we only know what it is) as the stone, or the acarus, or the tapeworm.

Thirdly, Admitting that Hahnemann's theory of disease and of cure in general were as sound as I have shown it to be unsound, the special application of it is a *petitio principii,* or unproved assumption, of the most gross and indefensible kind. It amounts to this, that as the Author of nature has established for our guidance a relation between the symptoms of natural diseases and the artificial symptoms excited by remedies, it is our duty to discover this relation; but as the two classes of symptoms (according to Hahnemann) must be either *similar, dissimilar,* or *contrary,* our choice of remedies must also be either homœopathic, allopathic, or antipathic. Both of the latter, however, have been tried and found wanting; therefore the former is necessarily, exclusively, and universally true.

Such is the argument. To every step in it I object. Not to press for the present the consideration that this argument is, at best, a leap in the dark from allopathy and antipathy, presumed on insufficient grounds to be false, into homœopathy, presumed on equally insufficient grounds to be true, I protest against the endeavour to exhaust the possible relation of symptoms to each other in the above three formulas. The experienced and scientific physician well knows that relations of the most unquestionable and definite kind have been discovered between symptoms,[1] which are not to be expressed in the idea of mere likeness or unlikeness; just as the mathematician knows that there exist among numbers relations of the most complex and exact kind, which are not to be expressed in the old and crude Pythagorean formulæ of equal and unequal, male and female, generated and not generated. These relations are, in both cases, the object of legitimate science. But the science of *pathological* relations is repudiated by the homœopathist, who shuts himself up in the prison of his own crude conceptions, and puzzles himself in the vague search after mere analogy, the most lax and fluctuating of all foundations for scientific knowledge.[1] The law of homœopathy could only be proved in theory, according to the scheme of Hahnemann, by disproving every other possible relation between the symptoms of the remedy and the symptoms of the disease. But so far from doing this, he has not disproved, any more than he has hitherto proved, anything at all. He has assumed much, he has guessed at more, and he ends with making the assertion that homœopathy must be true because allopathy and antipathy are false. And this is what we are told to consider a logical exposition of the homœopathic law, independently of experiment!

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[1] For example:—Blindness is a symptom or a form of disease. Of course, one case of blindness may be said to be similar to another case of blindness; and in the ordinary homœopathic sense it can scarcely be said to be similar to anything else. It is not very similar to a case of deafness, or loss of smell, or of taste, or of touch, or of motion. Yet, with all of these, it may present pathological relations of the most close and intimate kind. *Given*, several cases of blindness. The patients are all deprived, or partially deprived, of the light of day; the forms and colours of objects, the gay mantle of nature, the varied works of art, are shut out from the conceptions of all; they are "similar" cases. But one patient may be blind from cataract, one from palsy of the retina, one from inflammation of the cornea, one from adhesion of the pupil, one from tumour of the brain, one from total destruction of the eyeball. How much more really unlike each other are these "similar" cases than many others which, to the common view, present no such similarity at all? Again:—How much more resemblance between a case of blindness and lameness, both dependent on diseased brain, than between the former, and a case of blindness from cataract?

I have next to consider the homœopathic law in the light of positive experience. But here my limits forbid me to enter at all into the discussions which have arisen upon almost every one of those instances which have been adduced by Hahnemann himself, and by others, as the *instantiæ crucis.* I have already alluded to the ease with which vague analogies may be converted to the support of a preconceived opinion. The controversies in question afford constant proof of this tendency, which Dr. Simpson has happily compared to the easy and courtier-like acquiescence of Polonius in the doctrine "*similia similibus,*" when proposed to him by Hamlet:—"Do you see yonder cloud that's almost in the shape of a camel?" "By the mass, and it is like a camel indeed." "Methinks it is like a weasel." "It is backed like a weasel." "Or like a whale?" "Very like a whale." Any one who is at all accustomed to weigh medical testimony in relation to the therapeutical effects of remedies, and who will read over the introductory chapters of the *Organon* of Hahnemann, or any considerable passage of his *Materia Medica*, cannot, I think, fail to wonder at the laxity with which facts are admitted and conclusions set down upon authority, and under circumstances of contradiction and doubt which would damage a case of much stronger character than I have shown homœopathy *prima facie* to be.

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[1] In the very great difficulty of dealing with analogy as a principle for the establishment of the truth may be found the explanation of the fact that homœopathists rarely meet with any insuperable obstacle either to drawing their own conclusions from apparently adverse instances, or to explaining away the cases where homœopathy has been alleged notoriously to fail. Who can tell when a remedy has been homœopathically chosen? I have shown, in the preceding note, that the most dissimilar things may appear similar, and *vice versa*, from a slight change in the point of view. The more complex the symptoms, of course, the more involved and difficult the argument.

When, for instance, we are told that a fatty discharge from the intestine is cured by olive oil; that measles cured a chronic herpetic eruption, and was found a preventive of hooping-cough; that small-pox is a homœopathic cure for inflammation of the eyes, blindness (paralysis of retina), deafness, difficulty of breathing, dysentery, etc.; that arnica is adapted for the healing of wounds and contusions, because it produces stitches in the side, sickness, anxiety, and other nervous symptoms which *sometimes accompany* wounds and bruises ; that belladonna is said to have cured hydrophobia (?) and to produce symptoms in the healthy *somewhat* like hydrophobia (about as like as the cloud to the whale); that squill cures pleurisy (?), because J. C. Wagner saw " a sort of pleurisy " come on under this commonest of all remedies (which was never accused of such a thing before or since); that camphor cures typhus fever (*febris nervosa lenta* of Huxham), because it produces insensibility, prostration, and diminished temperature in poisonous doses; and that it also cures cholera (I presume for similar reasons); that wine is a good (homœopathic) remedy for inflammation (!)—(Crivellati, Augenius, Mundella, two other unnamed persons, and Asclepiades, are adduced as the authorities); when we hear these things we are tempted to ask, what doctrine could possibly be named so absurd as not to admit of something like evidence on similar terms ? But the climax is not yet reached. Hahnemann gravely proposes, as illustrations of the homœopathic law, the effect of the sunlight in extinguishing the brilliancy of the stars ; of snuff in removing the impression of fetid odours ; of the drum in preventing an army from being *terrified* by the roar of distant cannon; and other examples equally convincing!

Perhaps the most celebrated instances of the homœopathic law given by Hahnemann himself, and adopted with tolerable unanimity by his followers, have been these three:—Quinine, or Peruvian bark, against intermittent fever or ague; belladonna, against scarlet fever; and vaccination, in reference to small-pox. In regard to these three instances I shall make a few very brief remarks, without venturing on a detailed consideration of any of them.

The utmost that is asserted with any show of probability in the case of quinine is that, when taken in considerable quantity in a healthy person, it is apt to induce *feverishness* and *headache,* with *furred tongue* and *nausea.* On these grounds, which are more or less plausibly maintained, it is asserted that quinine is homœopathic to ague. Here, however, it is to be observed that, on the same grounds on which quinine is asserted to be homœopathic to ague, it may be with equal propriety maintained that it is homœopathic to ephemeral fevers of all kinds, and even, inasmuch as all fevers are "similar" in these respects to the alleged action of quinine, that its curative power ought to extend very widely over the range of febrile diseases; whereas we know that this therapeutic value of quinine, or of Peruvian bark, is actually circumscribed within very narrow limits; these remedies being quite as useless in most other fevers as they are useful in those of the aguish type, to which in reality their effects on the healthy system bear no *special* resemblance whatever. The whole case, therefore, as regards quinine is this—that a substance which, in very large doses, *occasionally* produces in the healthy feverishness, along with other effects, is applicable in the sick to the cure of one particular class of fevers only.[1]

The power of belladonna to prevent scarlatina when epidemic, and sometimes to cure it, was maintained by Hahnemann soon after the celebrated discovery of Jenner.[2] Now, the homœopathic character of this instance depends upon two circumstances, both of which are, unluckily, after years of controversy and experiment upon both sides, still rather more than doubtful. The power of belladonna to produce a morbid state having any considerable resemblance to scarlatina,[1] is one so rarely observed, if it really exist, that many physicians have used belladonna largely both in acute and chronic disease, and not unfrequently also in healthy persons, without once seeing such an effect. On the other hand, the alleged action of the remedy as a prophylactic in that disease has been pretty clearly shown to be quite visionary.[2]

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[1] *Note on the above*, 1888.—The more extensive use of quinine as an antipyretic in these later days cannot be said to invalidate this reasoning, but rather to confirm it. For, in the first place, whatever of benefit has accrued, or may yet accrue, to the medical art from the administration of quinine in enormously high doses to beat down excesses of bodily temperature (and I am not to be considered as advocating this employment of the remedy unreservedly) is unquestionably *not* homœopathic in principle; and, secondly, the much more extended, indeed almost daily, use of quinine by physicians now-a-days in very sensible or large doses, makes it absolutely certain that its *ague-producing* power is *nowhere*—a creature of the homœopathic imagination.

[2] It is rather unfortunate, moreover, that in two instances at least, this being one of them, Hahnemann was accused of vending a *nostrum* or secret remedy for his own personal profit; revealing it, in this instance, only when his conduct was openly denounced, two years afterwards. See "A Triad of System Builders," in a volume published by me in 1856, entitled "*Medicine and Medical Education*," p. 98.

The admitted power of vaccination as a prophylactic or preservative remedy against small-pox is claimed by Hahnemann as an illustration of the homœopathic law. Let us see by what reasoning this claim is maintained; premising that the most trustworthy medical authorities are content, in the first place, to admit the fact as a fact proved by experience; and in the second place, to connect it with the very reasonable, though not by any means certain, theoretical argument, that the vaccine disease *is*, in fact, small-pox under a modified form—less dangerous, both to the patient and to others—strictly local and non-infectious under all ordinary circumstances—but identical in essence with its prototype, and therefore carrying (as small-pox itself would do, and as most other contagious fevers do) the mysterious but well-ascertained power of protecting, within certain limits, against a renewed attack. If this theoretical explanation of an admitted fact be unsatisfactory (and I shall not presume to assert the contrary), what shall we say of Hahnemann's, which denies to the vaccine disease *identity* with small-pox, but asserts that it is *similar*, and because similar, curative? It is to me very clear that if the vaccine disease do not stand to small-pox in a relation of *pathological* identity, or similarity, that is, a relation founded on a common or a nearly common cause, it is scarcely possible to find two diseases, or at least two eruptive diseases, more widely different in that totality of symptoms to which Hahnemann refers as alone giving *homœopathic* similarity. Nay more, I maintain, and I think most practical physicians will bear me out in asserting, that there are diseases having greatly more resemblance, as regards symptoms, to small-pox than vaccinia has; diseases which are apt to be mistaken for small-pox (which vaccinia never is), even after close observation for some days, but which are not in any degree protective, and which may be followed by the true small-pox (if the patient be exposed to its infection), within a very few days. Further, Hahnemann admits that vaccination *does not cure small-pox when present.* It does not, therefore, perform the true office of a homœopathic remedy, according to the homœopathic law; it performs a totally different office, an office which homœopathic remedies generally do not perform; while these do possess, according to Hahnemann, that curative virtue which the vaccine disease confessedly has not. "To produce this effect (of cure)," says Hahnemann, "it *only wants that excess of power* which ought to accompany the homœopathic resemblance in order to effect a cure." He goes on to explain that, although deficient in power to cure, it is powerful enough to excite a disease similar to small-pox, and so similar as to possess even that most mysterious and inexplicable of all the qualities of small-pox, protective virtue against a recurrence of the disease. Let those accept this explanation who will; to me it seems the mere jangling of meaningless words, alike incapable and undeserving of being treated as an argument.

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[1] We have seen above that belladonna is likewise homœopathic to hydrophobia. Now, as Hahnemann always professes to judge of the application of a remedy by the *totality* of the symptoms, and as there are few diseases more unlike than scarlatina and hydrophobia, it is difficult to understand this double curative action of belladonna.

[2] See an article by Dr. Warburton Begbie in the *British and Foreign Med. Chir. Review*, Jan. 1855, p. 77; and the succeeding controversy in the *Homœopathic Journal* and *Monthly Journal of Medical Science* for 1855.

But suppose that in these three instances, or in many more instances, a similarity between the action of the remedy and the symptoms of the disease had been pointed out, would that similarity establish the homœopathic principle, or law, in therapeutics? Most certainly not; because, amid the complex phenomena of disease on the one hand, and of remedial actions on the other, it can hardly happen otherwise than that there should be accidental resemblances; as there is, for example, between the action of an emetic and the sickness caused by the surfeit for which the emetic is plainly the proper remedy. To establish the homœopathic principle as regards this particular symptom, it would be necessary to prove, that in *all* cases of stomach-sickness the cure is to be sought in the artificial use of substances tending in the healthy to stomach-sickness or something like it; and that the more sick the stomach the more is the homœopathic remedy in question indicated. The fact is, however, that the evidence of the homœopathic theory stands before us in "shreds and patches" only; we are never invited to take up any single class of symptoms, much less any single symptom, and to exhaust the consideration of it in relation to its appropriate remedy or remedies; on the contrary, we are asked to view symptoms in their most intricate combinations, and to seek for traces of similarity between the *totality* of the symptoms ascribed to the disease, and the *totality* of the symptoms ascribed to the remedy. Let any one, keeping this in view, look at a homœopathic work on the Materia Medica on the one hand, and a somewhat elaborate and learned exposition of nosological science on the other, and he will be at no loss to discover how systems are built up in medicine, and why they are of so unstable a character when built. The pedantry and cumulative labour of ages have given to each disease a farrago of characters of almost hopeless complexity; and the imagination of homœopathic "provers," which has ascribed to charcoal 930 symptoms, to oyster-shell, 1090, to cinchona bark, 1143, has made the search after illustrations of the homœopathic law merely a matter of time and industry. To seek either the establishment or the rejection of that law by individual instances, therefore, seems to be a very unsatisfactory way of arriving at the truth. And with these remarks I close this part of the investigation.

II. *Examination of the Homœopathic Practice.*—Is homœopathic practice generally, or in particular hands, so successful and so consistent with the principles on which it professes to be founded, as to form a ground for admitting those principles to be true, independently of the evidence which we have already discussed? Such is the inquiry to which I now proceed.

The inquiry is one of considerable difficulty; but it may tend to lessen that difficulty if we observe that homœopathy must do much more than prove a *moderate* amount of success in order to entitle it to consideration from its own point of view. If homœopathy be that universal, sure, and trustworthy system which it claims to be, and if it be opposed to a system radically wrong in theory, and murderous in practice, as Hahnemann asserted, the difference between the results of the two methods ought to be the difference between food and poison, between light and darkness, between absolute success and utter failure. Further, we have a right to expect, as a legitimate consequence of the position claimed by homœopathy, that its professors shall have followed out its principles, and shall not shrink from their application in practice, either in acute or chronic, in severe or trivial diseases. With these preliminary remarks, I proceed to consider, in the first place, some of the practical results obtained by Hahnemann, as recorded by himself.

The first point to which it seems necessary to direct attention, is the degree of confidence with which Hahnemann carried out in practice his preconceived law of homœopathy, and the range of diseases over which he extended it. There seems no reason to doubt that Hahnemann rigidly abstained from anything at all approaching to the ordinary use of drugs. At least, I am not disposed to question his statements on this point. That he was very careful and precise in his dietetic regulations, and caused his patients to adopt a very strict regimen, is generally admitted; and that his energy of character, high pretensions, and great celebrity, induced many of them to repose unbounded confidence in him, is also more than probable. That he possessed, with a degree of that metaphysical haze so prevalent in the German mind, uncommon tact in shaping his course according to circumstances, is clear from every part of his writings. I think, therefore, that, *apart from his use of medicines,* Hahnemann was probably a successful practitioner in a considerable proportion of the cases in which he was ordinarily consulted. This success he had in common with all other practitioners who obtain the confidence of their patients, and establish for them fitting rules of life.

When we come to consider his use of remedial agents, the first peculiarity, beyond the homœopathic law itself, which strikes us is the tenuity of the doses. The method of infinitesimal doses, as it is called, was not originally part of the homœopathic system, with which it has apparently no very obvious or necessary connection; nevertheless, it formed in the end a part of Hahnemann's creed, to which he was not less devoted than to the law of "*similia similibus curentur*" itself. The system of minute doses has moreover been embraced by most of Hahnemann's followers, some of whom, indeed, have pursued the idea of attenuation to a degree almost inconceivably beyond the most inconceivable of the original infinitesimal dilutions, while others have tended to revert towards more palpable quantities. I shall not follow out all the details bearing upon the process of attenuation alluded to; they are for the most part well known. A grain of the medicine is dissolved in 99 grains of alcohol, or alcohol and water; then one drop of this solution (1st dilution) is mixed with 99 drops more of alcohol; one drop of this further attenuation (2d dilution) with 99 drops more of alcohol; and so on to the 10th, 20th, 30th, or 600th to 1000th dilution as the case may be. The result of this extraordinary and inconceivable process of subdivision is stated by Dr. Simpson in the form of a calculation revised by competent mathematical authorities, and from which I shall give only the following illustrations:—At the 6th dilution, had the entire original grain been furnished with its proper amount of alcohol, that quantity could have amounted to 13,000,000 gallons; at the 12th dilution, to a sea six times the size of the Mediterranean; at the 15th, to 46,000 times the whole waters contained in all the oceans of the globe; at the 30th dilution (the ordinary one employed by Hahnemann, but very much below those recommended by some of his followers), to a "quantity sufficient to make one hundred and forty billion spherical masses extending from limit to limit of Neptune's orbit, or a quantity equal to many hundred spheres, each with a radius extending from the earth to the nearest fixed star."[1]

A consideration of this strange doctrine (which was only feebly expressed by Lord Jeffrey when he said that an ounce of medicine, put into the Rhone at the upper end of the lake of Geneva, would physic all the

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[1] *Homœopathy, its Tenets and Tendencies;* by Professor Simpson, p. 285.

Calvinists at the lower end,) excites a natural curiosity to know how Hahnemann was led so far (apparently) out of his way. The answer to this question is important.

Hahnemann did not fail to discover, as he has told us in the *Organon*, § 276, *et seq.*, that a homœopathic remedy "*always does harm when given in too large a dose, and does harm all the more in proportion as the dose is larger.* Moreover, the *more homœopathic* the remedy, the greater is the injury from too large a dose; the effects of a homœopathic remedy, under these circumstances, *being much worse than a similar dose of a allopathic medicine.*" From this proposition Hahnemann was gradually led into a series of experiments (for he says it is impossible that theory could decide the matter) as to the necessary attenuation of a homœopathic remedy to avoid the consequence above indicated, viz., of its being more hurtful than an allopathic remedy in like quantity. The precise evil effects ascribed to the over dose of the homœopathic medicine are detailed elsewhere (*Organon*, § 156-7); they include not only the development of those powers of the drug which do not stand in exact relation with the symptoms, but in almost every case an *aggravation of the original symptoms;* or, as Hahnemann expresses it, "a small aggravation, which so closely resembles the primitive affection, that the patient himself supposes it to be an increase of his disease; whereas it is, in fact, a medicinal disease, very like the primitive disease, and surpassing it a little in intensity."

On the strength of these admissions, I claim Hahnemann as the first witness against his own system. It is plain that the reduction of the homœopathic dose to an insensible and preposterous minuteness, was determined by a fact which ought in reality to have determined the rejection of the system, founded, as we have seen it to be, not upon experience, but upon abstract reasoning. The first experiments, doubtless, revealed what subsequent experiments corroborated—that homœopathic remedies invariably aggravate the diseases they are intended to cure;[1] or, as he somewhat naïvely says, that "the artificial disease which the remedy has excited in the most suffering parts of the organism, goes the length of being hurtful." Hence the necessity of reducing the doses. And once involved in this train of false inference, Hahnemann did not hesitate to spring from one absurdity to another. The idea, indeed, that the curative virtues of remedies are *spiritual* or *immaterial*, led him to look on them as bearing no fixed relation to the bulk of the medicine; and all the nonsense about the influence of shaking and trituration in developing these "dynamic virtues," naturally followed.

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[1] Hering, a homœopathic expositor of Hahnemann's doctrines, is so distinct and concise in his statement on this point, that I quote the following passage, as given in the work of Dr. Epps, *Homœopathy and its Principles.* The "almost constant aggravation of the disease by the remedies which were chosen according to the new law, threatened to embarrass very much their trial, if not to render it wholly impracticable. To avoid these disagreeable results, Hahnemann adopted the most simple and natural expedient, viz., that of lessening the dose."

Another point in which Hahnemann's system evidently gave away beneath his feet, was in relation to chronic organic diseases. It was impossible to assert that there existed any remedy directly homœopathic to cancer, caries, disease of the vertebræ, etc., etc. The consequence of this vital defect in the system (in connection with the general idea that remedies *must* be found for every disease) was the invention of a class of *antipsoric* remedies, as he called them, and the celebrated "*psoric hypothesis*," which even Dr. Henderson, who, after a fashion, defends it, calls in one place, "perhaps the most unfortunate of his speculations." By this hypothesis, for which Hahnemann takes to himself immense credit,[1] he refers all chronic diseases indiscriminately, with the exception of those due to syphilis and sycosis, and a few others, to a *special chronic miasm*, on which he confers the ancient name "*psora*." This *psora* is the "single source of innumerable morbid forms," and is to be attacked chiefly by a single remedy, sulphur, which Hahnemann maintained to be antipsoric, and homœopathic to the miasm of psora,

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[1] "It has cost me twelve years of researches," says Hahnemann, "to discover this great truth, which was *unknown to all my predecessors and contemporaries*."—*Organon*, § 80, note.

though not presenting the usual homœopathic relation to the multiform symptoms of this disease.

In the case of the psoric hypothesis, there can be no doubt that Hahnemann falls into exactly the same course of reasoning which he inveighs against in the case of the allopathist—searching into the hidden causes of diseases, and placing the remedy not in relation with the manifest symptoms, but with the supposed nature of the disease.

On comparing, therefore, Hahnemann's practice with his theory, I find the latter to be contradicted by the former; firstly, because the theory of infinitesimal doses is clearly only a gilded mask to cover the bad effects which he is himself obliged to ascribe to sensible doses of homœopathic remedies; secondly, because the psoric hypothesis is clearly an invasion of the homœopathic principle in relation to a vast tribe of diseases, which it is to be presumed were rebellious to the homœopathic law. I therefore claim Hahnemann's practice unhesitatingly as a refutation of his own system; and ascribe so much of its success as was real to the operations of nature, aided by the hygienic, and especially the dietetic and mental influences to which I have adverted above. To the infinitesimal doses, *as medicines,* I ascribe simply no effect whatever.

Having thus examined my subject from every point of view to which there is access in the writings of Hahnemann himself; having carefully weighed the claims of homœopathy to consideration, both as a system of doctrine and as a method of practice; having found it to be, on the one hand, based upon assumptions destitute of proof, and inconsistent with known facts; while, on the other, it stands condemned by the very experimental evidence which has been adduced in its support, I might perhaps bring this inquiry to a conclusion. But it cannot fail to have struck those who are familiar with the position of homœopathy in the present day, that there is an obvious disposition, on the part of its adherents, to avoid the discussion of principles and of elementary facts, and to invite attention chiefly to the general results of practice in homœopathic hands, as the only true test of the value of the system. We hear every day of wonderful cures of isolated cases; of desperate diseases which yield to homœopathy in a way unknown to the old practice; of homœopathic hospitals in which the ratio of mortality is surprisingly low, and in which the most acute and formidable diseases are cured in the most astonishing manner. Statistical tables are presented to us showing the results of treatment in homœopathic and in "allopathic" institutions; and we are told that whatever becomes of the homœopathic law, a system must be known by its fruits, and of these the public can judge for themselves from the facts presented. Besides this, the horrors of bleeding and blistering, bolus and draught, powder and pill, are constantly brought before us, and contrasted with the amenity and the slight disturbance to the system of the small sugared globules, or the colourless and tasteless solutions in which the infinitesimals of the homœopathist are diffused. And upon these materials the public is invited to give its verdict, while the homœopathic law is thrown into the background.

I am in no way disposed to shrink from this aspect of the question, any more than from the others which have been already discussed. But I take leave at the outset, before stating the very few considerations for which there is room, to remark, that if there be anything which requires the exercise of a skilful judgment, and a specially educated critical faculty, it is precisely a question of this nature—one involving considerations of the most delicate, and moreover the most technical kind that it is possible to conceive. To refer the question between two systems of treatment, therefore, to "common sense" (as is done by a noted homœopathic authority, in relation to an elaborate statistical challenge, and assuming, of course, the general accuracy of the statistics produced), implies a degree of insensibility to the true conditions of the inquiry, which only the blinding influence of party could possibly produce.

A striking exhibition of the fallacies inherent in statistical statements of this kind, put forward in the interest of a system, is afforded by an examination of the results of treatment in the Vienna Homœopathic

Hospital. It is now some years since I had occasion to publish an analysis of the reports of that much vaunted institution; and I am not aware of any reply to my criticisms in the least deserving of, or requiring, notice. It had been customary for the homœopathic writers and journalists to point triumphantly to these reports, as indicating results "far beyond the reach of any other mode of treatment;" and this on two grounds, 1st, that the general mortality was 6¼ per cent., which was vaunted as surprisingly low; 2nd, that in particular, pneumonia, pleurisy, peritonitis, and other acute diseases, were treated with a success unknown in other hospitals. So prevalent was the idea that these statistics were unassailable, that several writers not favourable to homœopathy were led into giving almost unlimited credit to their results. Yet so completely has this credit broken down, as regards the first point at least, that Dr. Henderson himself (while defending as it were, every inch of defensible ground) now admits the unfairness of the original comparison instituted by the homœopathists on this point.[1] The homœopathic hospitals have, in fact, by a somewhat strict comparison with those of ordinary physic, been shown to be systematically filled with those trivial and easily curable cases, which all considerable and well-filled hospitals are compelled, by the pressure of severe and fatal disease, to exclude. Thus, in the Vienna Homœopathic

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[1] *Homœopathy Fairly Represented*, p. 82.

Hospital, consumption and organic disease of the heart, liver, and kidney, find place in exceedingly small proportion; while colic, diarrhœa, headache, sore-throat, chicken-pox, affections of the skin of an acute and curable kind, and similar diseases of other organs, swell the lists to a degree unknown in any considerable hospital established for purely charitable purposes. To give but one or two instances—out of about 6500 cases, the accumulation of several years, the *homœopathic* hospital at Vienna had 300 of sore-throat; while the *general* hospital of Vienna, admitting not less than 21,400 cases in a single year, had, in this enormous number, only 216 cases of inflammation connected with the mouth, gums, teeth, palate or tonsils, or about one third less than the cases of sore-throat alone in the homœopathic institution. Again, in the homœopathic hospital, there were 110 cases of chicken-pox, while in the Edinburgh Infirmary, there were only 2, out of a nearly equal aggregate of cases. On the other hand, for 276 cases of consumption in Edinburgh, there were only 98 so called in the Vienna Homœopathic Hospital; for 159 cases of diseased heart, 15; for 82 cases of diseased kidney, *none;* for 103 of paralysis, 5; for 33 of disease of the liver, 1; for 17 of diabetes, *none;* for 14 of neuralgia, *none;* for 18 of internal aneurism, 1; and so on. Yet with this very remarkable selection of cases, the mortality was actually higher by one third than that of the average of English provincial hospitals; and was in fact shown to be only exceeded by those hospitals, in this country and abroad, which, from their situation and other causes, receive an enormous proportion of severe and incurable cases.[1]

Then, as regards the cases of acute disease supposed to have been treated with such surprising success in the Homœopathic Hospital, one fact may suffice to indicate the bias in the numerical statements. All the world over, catarrh of the chest or bronchitis is a very frequent disease, while pneumonia, as well as pleurisy, is comparatively rare. Vienna is no exception to this rule; for, in the General Hospital of that city, there are admitted 2078 cases of catarrh as against 509 of pneumonia, and 427 of pleurisy. In the Homœopathic Hospital, on the contrary, the proportion is so completely inverted, that for 300 cases of pneumonia and 224 of pleurisy, there are only 15 of acute bronchitis mentioned, *while all the other diseases of the chest together do not make up nearly the difference;*[2] thus showing clearly that every example of curable disease which could, however violently, be twisted into the semblance of pneumonia or pleurisy, was made to swell the list of these two formidable diseases, and thus to increase the apparent success of the homœopathic experiment. The case is even more curious as regards peritonitis, a disease undoubtedly rare both in hospital and general practice; of which, nevertheless, 105 cases appear among the 6000; the Edinburgh Infirmary showing in a similar total number only 19 of peritonitis.

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[1] For the more full statement of the whole question, see the author's article on "Homœopathic Hospital Statistics," in the *Medical Times* for April 3rd, 1852, p. 341.

[2] I have accentuated this clause by italics, in order to ask the medical reader to give full effect to it in its obvious meaning as bearing on the statistical question at issue; thereby avoiding the revival of a threshed-out controversy founded upon a clerical error in my original paper in the *Medical Times and Gazette*, 1852. After most carefully considering everything written

These remarks will, I trust, suffice to show how very unsatisfactory are the results to be deduced from the examination of either hospital or private practice as bearing upon the success of homœopathy. Nevertheless, I am not by any means inclined to deny that homœopathic practice is frequently successful in the treatment of disease; nay, that in certain cases, its success as measured by the result, may be not less than that of judicious, and a great deal more than that of injudicious, practice of the ordinary kind. A very large proportion of cases require the assistance of the physician simply as a skilled adviser; and that man is no true physician who does not quite as frequently dissuade from active drugs, as urge the patient to their use. In this class of cases

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upon the subject then and since, I desire it to be understood that the argument as submitted in 1856, and here reproduced, is, in my opinion, final and unassailable. October, 1888.

the most rigid homœopathist, with his infinitesimal doses, is on a par, as regards medicine, with the physician, and possibly has even an advantage over him, if the faith in globules be, on the part of the patient, deeply rooted. But between the homœopathist, in such a case as this, and the unhappy apothecary who, from old habit, or for present emolument, will insist on drenching, and sickening, and narcotizing his patient alternately, after the routine manner still somewhat too common, and so well hit off by Molière in the *Malade Imaginaire*,[1] there is no comparison at all. The public will always play off the man who does nothing under a fine name, against the man who under a fine name unnecessarily tortures its bowels, drains its blood, and decorticates its exterior. And, no doubt, it is on the whole well both for medicine and its professors, that the Sangrado should, in the natural course of things, evolve the Hahnemannist with his infinitesimals, just as the curmudgeon of a father evolves the spendthrift of a son, as the Puritans evolved Charles II. and Nell Gwynne, as the "Grand Monarque" sowed the seeds of the French Revolution. We may be assured that nature's laws, in the remedy of abuses and absurdities, are not homœopathic, nor infinitesimal neither; it is not "*similia similibus*" but "*contraria contrariis curantur.*"

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[1] "Clysterium donare
Postea seignare
Ensuita purgare
Repurgare, reseignare, et reclysterizare."
Le Malade Imaginaire, Troisième Intermède.

The time when Hahnemann lived and wrote was singularly favourable for the first propagation of such a theory as his. The century preceding had been eminently an era of systems and of hypotheses, each one more crude, and at the same time more fantastic than its predecessor. The Iatro-chemists had given way to the Iatro-mathematicians, salt and tartar to lines and triangles, and the attempt to reduce disease to a geometrical demonstration; humoralism had been shaken to its foundations by Hoffmann with his theory of spasm; and Boerhaave, with immense labour and erudition, had attempted to construct a quite too ingenious eclectic system, in which "*error loci*" and "spasm of the vessels" should hold a place beside "the spontaneous gluten," the "acid and alkali," as proximate causes of disease. But the end was felt by the best minds to be vanity of vanities. The pathological knowledge of which the basis was laid by Berzelius in the chemical department, and Morgagni, Hunter, Bichat, Corvisart, Bayle, and Laennec, in the anatomy of disease, was wanting to initiate a real advance. Hahnemann's speculation, therefore, fell upon a soil barren of new facts and teeming with old and worn-out hypotheses. It was one of a thousand theories, but it suited the temper of some men's minds, both by what it professed, and by what it rejected and abandoned. It was scarcely more absurd than the Brunonian system of medicine, and much less mischievous. It was in fact less injurious in practice than almost any of the exclusive theories which either preceded or succeeded it, and therefore it found favour with those with whom an exclusive theory was a necessity, but who had seen the mischief of the current hypotheses.

Among those who call themselves homœopathists in the present day, there are not a few who depart widely from the principles of Hahnemann himself, and not only administer what he called allopathic or antipathic remedies, but do so habitually, and confessedly with reference to the occurrence of cases in practice in which "the homœopathic principle cannot be of service to them, whether from its own present or necessary limitations, or their insufficient acquaintance with it."[1] Of course, from the moment when the homœopathic principle is regarded as subject to "necessary limitations," it ceases to take rank as a law of nature; for to nature's laws there can be no real, however there may be apparent, exceptions; and it is not permitted to us to violate them, even in the way of omission, with impunity; much less to set up in opposition to them, in particular cases, principles of a contradictory character, or maxims founded on special, and therefore limited, experience. If, in so common a case, for instance, as constipation of the bowels, it is habitually found impossible to relieve the symptom in question by the administration of any of the numerous substances known as capable of producing a like condition in the healthy, and if, on the contrary, the adoption of laxative medicines, or of a diet and regimen tending *in the healthy* to relax the bowels, be habitually resorted to in place of, or in addition to, homœopathic remedies, then is homœopathy (as regards one very large class of instances) proved to be false; and therefore, if for no other reason, is the homœopathic law proved to be no law of nature. The system of Hahnemann himself is at least entitled to examination as a whole, on the footing of logical consistency; but with those who, ranging themselves under his banner, and adopting some of his formulæ, take shelter from the shower of opposing facts under the doctrine of exceptions and "necessary limitations," it is impossible to hold any argument about the homœopathic law.

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[1] Henderson's *Homœopathy Fairly Represented*, p. 25.

The author whom we have quoted above claims for himself and for those who act and think with him the credit of "professional and scientific catholicity." If this only means that such persons are open to conviction as to the best course to be followed in each special case, argued on its own merits, and without reference to any general therapeutical law, such as the homœopathic law professes to be, then we are at one with them; but then, also, they cease to be homœopathists in anything but the name; and the questions between them and the medical world in general are of a very limited description—simply, indeed, the questions discussed in every medical journal, and in every lecture-room, with regard to the effects of particular remedies, or modes of treatment, in particular diseases. For it must always be recollected that the scientific physician of the present day affirms no general law of therapeutics, nor does he repudiate homœopathic remedies in detail; he admits that the medical art consists in a calculation of data peculiar to each disease, or class of diseases, and often to each individual case; he admits, *as a fact*, that some of his remedies produce physiological effects similar, and many other dissimilar or opposite, to those of the diseases for the cure of which they are found to be adapted; but he denies that these circumstances form a legitimate ground for the adoption of homœopathy on the one hand, or of allopathy or antipathy on the other. From all such universal systems he recoils, knowing that they are not only indefensible and crude in theory, but calculated to communicate an injurious bias to practice; but they tend to exclude from view important considerations, founded on the plainest dictates of experience, and the most obvious applications of common sense; that they seduce the reason by a false show of simplicity, and confine the judgment within a vicious circle of propositions, not less doubtful in fact than they are dangerous in application. The true physician derives his chief claim to that character from his perfect intellectual and moral freedom. Untrammelled by the shibboleth of a party, and undisturbed by the fantastic hypotheses by which men seek to reduce the variety of nature to a false semblance of unity, he is able to devote his best attention to those practical questions which emerge at the bedside, and not in the study. He finds these questions, small and peddling as they may seem to minds intoxicated with science or with learning, to be of very vast importance, and to involve in their answer a very solemn responsibility. He seeks their answer, accordingly, from all available sources;—from previous personal experience, from credible and unbiased testimony, from cautious induction and analogy, from legitimate theory, finally from the march of events in each special case, watched, if necessary, from hour to hour, and from minute to minute;—from all these sources he gathers the elements of a judgment, which he rarely submits to revision on the ground merely of a remote or abstract hypothesis. The questions which he has to resolve are often obscure and difficult, the results at which he arrives confessedly uncertain, perhaps often erroneous; but they are questions which *must* be met, results which *cannot* be evaded. His concern for his patient, therefore, and his own peace of mind, equally demand that he shall occupy a position free from sectarian bias, and from the narrow bigotry of a stereotyped therapeutical creed. It is his business to use, not the remedies of this or that individual, this or that party, but those remedies which experience and science combine in recommending as capable of affecting the specific purpose for which he uses them. If the use of those remedies be further justified by a satisfactory theory of their action, it is well; but, if not, he is content to wait for further light, and to reap, in the meantime, an advantage the nature of which he does not profess to understand. True science and safe practice require that he should not only desire knowledge, but should be free from all suspicion of presumptuously over-leaping the barrier which divides our knowledge from our ignorance.

It is the misfortune of homœopathy that its votaries, like those of all exclusive sects, never can occupy the position above defined as that of the true physician. If, indeed, their leading principle had been an axiom, or self-evident truth, its exclusiveness might have been justified. But even in this case it had better have been tacitly assumed or calmly maintained against discussion, than have been made the ground-work of those virulent attacks upon the convictions of others, which, as we have seen, formed the most prominent part of Hahnemann's literary labours. By ungenerous and unfair treatment of his adversaries, by an arrogance and presumption which, if not allied to madness, are indicative of the most insufferable personal vanity, and by a mode of dealing with facts which shows but a very slender regard for the modest limits of truth, Hahnemann from the first placed himself beyond the pale of ordinary scientific discussion, and provoked retaliation, both of a moral and a material kind, which I am not prepared in all cases to justify. That a similar spirit of self-exaltation on the one hand, and a somewhat similar disposition to make reprisals on the other, still continue to characterize this unhappy controversy, is but too true. Indeed it is obviously part of the policy of homœopathy to encourage this strife; to flaunt in the faces of physicians the banner of its exclusive dogma, and to perpetuate the use of nicknames associated with every kind of bitterness. I have been desirous, in this article, of adding as little as may be to the exasperation of the contending parties. Although in duty bound to state clearly and strongly my own opinions, I have endeavoured to do so, without distorting or calumniating those of the homœopathists, whom it has been my anxious desire to represent accurately according to their own authorized sources of information.

XI.—THE PRESENT ASPECT OF MEDICAL SCIENCE—OLD ORTHODOXY AND MODERN OPINION.[1]

And now let me consider for a little the studies in which you are about to engage—first, in their more strictly professional aspect; and next, as a section of the studies in various faculties taught in this ancient University. The art of medicine is at this moment in a peculiar position. The day of *orthodoxies* is over, the day of *real science* is only just dawning. It is no longer possible to condemn a man, even by implication, for having ceased to believe what our fathers believed; but it is extremely difficult to state in general terms what we believe ourselves, and still more difficult to forecast the future, and to lay the foundations of the faith of our successors.

I speak, of course, in these remarks, chiefly of those principles which bear most directly on medical practice.

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[1] Extract from an Introductory Address, delivered in the University of Glasgow, to the Students of Medicine of the Session 1866-67.

We all believe, no doubt, in the circulation of the blood. The doctrine which laid the foundation of Harvey's fame—which was set aside by Bacon as unworthy of notice, and keenly advocated by Descartes—which troubled, in various degrees, the souls of Riolan, and Primrose, and Gui Patin, and many others in the seventeenth century—is now a matter of assured conviction; not however as an orthodoxy, *i.e.*, an affirmation sustained by authority; but as a demonstrated fact, sustained by accurate investigation. No written or spoken creed is necessary to maintain this doctrine, simply because it stands upright and maintains itself to the convictions of all reasonable men. So also we all believe that lymph and chyle pass into the blood by the thoracic duct—that the blood contains iron as an essential part of its constitution—that the brain contains a large proportion of phosphorus, which to it is equally essential—that the nitrogen of our food passes into the blood as albumen and out of it as urea—that nerves are in part sensory and in part motor—that the tri-facial nerve is sensory and the portio dura of the seventh pair motor—that the gastric juice dissolves albuminous substances, and that the pancreatic fluid, assisted perhaps by the bile and the duodenal secretion, emulsionises fatty matters—that throughout the animal creation oxygen is absorbed from the air, and carbonic acid is given off to it—and that throughout the vegetable kingdom this process is mostly reversed. In all these instances we have well-ascertained and sure beliefs, founded upon exact researches, chiefly of the last half-century, and practically undisputed. But though true and assured beliefs, these things are not *orthodoxies*, because no one has a title to say to any one else, I insist that you believe so and so, or I will disown you as a professional brother.

When we look to the realm of pathology we find the circle of these assured beliefs much smaller, but still there are a few such, and the principle on which the belief rests is the same. That in fevers, there is increased temperature of the blood corresponding to increased waste of nitrogenous and other principles of nutrition; that in scurvy, there is a deficiency in certain imperfectly isolated principles of the food, represented by the acid juices of succulent vegetables and fruits; that in small-pox, a poison enters the system by the skin or by the breath, multiplies itself in the blood, and is thrown out in a concentrated form in the pustules of the disease; that the vaccine poison has certain curious relations to small-pox, in virtue of which it may be substituted for it in the human system, with the effect of protecting the individual so vaccinated from the more fatal disease; that in cholera, a more or less analogous poison enters, probably by the mouth, and after multiplying itself within the body is discharged by the intestines in a form capable of reproducing the disease; all these are examples of valuable and well ascertained truths, secured to us in some instances by extremely laborious researches, and capable of being turned to account in the relief of suffering humanity. And here again there is a general and indeed all but universal conviction, but no orthodoxy; the truths maintain themselves by their inherent stability, and therefore are not imposed upon any one by any external system of verbal coercion.

But, when we try to push this process of analysis a little further, and to express in definite words the beliefs under which we act in our daily practice, how much there is of imperfect knowledge, how little of secure and clearly defined conviction! Is blood-letting useful in pneumonia, or in apoplexy; and if so, in what cases, and to what extent? Are alcoholic liquors useful in fevers, under what circumstances, and to what extent? What is the cause, and what the preferable treatment of acute rheumatism? Is mercury to be given in pericarditis, or other acute heart disease? or in syphilis, and, if so, when and how? Are diuretics useful in Bright's disease, and when? Is opium needful in delirium tremens, and when? How would you treat acute dysentery? with calomel, or leeches, or ipecacuanha, or with opium, or with none of these? How would you treat cholera, with opium, or with purgatives, or with neither, and if with either, in what cases and under what conditions?—Who does not feel that in most of these instances questions have been started in which the old orthodoxies have been rudely shaken within the last twenty years, and in which nothing more definite or fixed can be alleged as having come in their place? Who does not feel that to set up a new creed in any of these particulars now, would be to insult the dignity of science, and to do incalculable injury to the cause of truth? For in almost every one of these instances the wise and skilled physician of fully formed experience has profited largely, and his patients have profited still more, by the destruction of the authoritative beliefs involved in most of these questions. He now acts freely in the light of his own convictions and well considered personal experience, where he was formerly bound in the chains of a traditional opinion. Now, at last, he can call his soul his own. There is no opinion so ancient or so general that he *must* receive it. There is no opinion so modern or so eccentric that he *must* perforce reject it. Between the old and the new, the sound and the unsound, the orthodoxy and the heresy, a thoroughly modern physician, if he be only honest and able, now stands as an impartial judge; unfettered, not seldom perplexed; but adding calmly to his store of real science and personal experience, to see if it will yield him here and there a few useful grains of secure truth.

MEDICAL ORTHODOXY AND HERESY IN THE OLDEN TIME.

It was not always thus. The inimitable plays of Molière, the essays of Montaigne, the marvellous satires of Rabelais, contain many traces of a period when the strife between orthodoxy and heresy was as fierce and as full of bigotries on both sides in medicine as it ever was in the still greater controversies of religion. The letters of Gui Patin, dean of the Faculty of Medicine of Paris in the middle of the seventeenth century, have stereotyped for us and for all time, with the most charming frankness and simplicity, a state of opinion in which the circulation of the blood was still an almost forbidden faith more than thirty years after the facts were made clearly known by Harvey;—a time in which all manner of chemical remedies were proscribed by a dominant opinion, as being unknown to Galen and Hippocrates;—in which bleeding and purging figured as the authorized remedies for nearly all diseases and all stages of them;—in which antimony and Peruvian bark were denounced with equal bitterness—the one as savouring of the "chemists," the other as the "powder of the Jesuits";—in which tea was an "impertinent novelty of the age," and opium nothing better than a poison;—in which, in short, the established orthodoxies of that particular era had to be maintained out and out, and were in fact maintained out and out, both as against old and new opinions—against Arabian traditions and modern innovations—with equal pertinacity; and, no doubt, in many instances, as in that of our charming and garrulous Gui Patin, with equal good faith.

There was scarcely anything awanting in the midst of these acrimonious controversies to complete and to justify the satirical picture of Thomas Diafoirus in the pages of Molière: a simpleton by nature, turned by art into a physician; drilled and tortured through a course of formal logic and pedantic learning into a regular round of fixed opinions; his mind "well broken in and laced up in Spanish boots" (as Goethe has it), to the utter destruction, not only of all originality, but of all desire for originality. "We had all the trouble in the world (such is the picture) to teach him to read; he was nine years old before he knew his A B C. . . . When I sent him to college he was much distressed at first, but he hardened himself against all difficulties, and his regents always praised him to me for his assiduity and for his work. At last, after many strokes of the hammer on the iron, he has gloriously attained his diploma; and I can say, without vanity, that for the two years during which he has been upon our benches no candidate has made more noise than he in all the disputations of our school. He has become formidable, in fact; there is not an act (*i.e.*, a public disputation) that he does not argue tooth and nail, right or wrong, for the contrary proposition. He is firm in disputing, strong as a Turk on his principles, never gives in an inch of his opinion, and pursues a verbal argument into the very uttermost recesses of logic. But, above everything, that which pleases me in him, and in which he follows my example, is that he attaches himself blindly to the opinions of our forefathers, and that he has never chosen either to comprehend or to listen to the reasons and the experiments of those pretended discoveries of our age touching the circulation of the blood, and other opinions of the same kidney." [1]

The old Faculty of Medicine fell, having done its work both of good and of evil; it could not survive in an era of experimental physiology; the circulation of the blood was too much in the end for all the learning and all the orthodoxies of that most learned and most orthodox age. Still, we must not be unjust. The Faculty was not always obstinate, nor always in the wrong; and even our honest Gui Patin, though prejudiced and bitter against what he thought to be quackery and causeless innovation, was by no means a Thomas Diafoirus. On the contrary, one sees in his letters everywhere the spirit of a true-hearted and genial physician, fettered, it is true, by traditional and authoritative opinions, but protesting with all his might against complicated and quackish remedies, bezoards, theriac, mithridate, as well as against all "occult qualities," alchemy, astrology, talismans, charms, and other manifest impostures. Nor can there be a doubt that his resistance to the progress of antimony and Peruvian bark was the testimony of a thoroughly honest and upright man against many and gross abuses in practice, upheld in the end by the sympathy of the Court, and, strange to say, by the support of that same Faculty with whose authority he had so often fortified his denunciations of Guénaut and the antimonialists.

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[1] *Le Malade Imaginaire*, Act 2, Sc. 6.

The history of this last dispute is curious, and may be here condensed into a very few sentences. In 1566, the Faculty of Medicine had taken solemn action in the case of antimony, which the extravagant praises of Paracelsus and the chemists had brought into a kind of popular repute. The decree was apparently unanimous, and was to the effect that "by the authority of all who have been distinguished in medicine, and for reasons already submitted to the king, *antimony is deleterious, and to be counted among the simples which possess the quality of poisons. Nor can it be amended by any other preparation, so as to be taken without injury.*"[1] In 1615, another decree placed several chemical drugs, antimony included, under formal condemnation of the Faculty, which, "by the unanimous consent of all," interdicted drug-vendors and all others from uttering these drugs; and "calls upon all judges to deal severely with those who prescribe, administer, or exhibit for sale the said medicines."[1] Both of these acts were ratified by the Parliament of Paris, and both of them continued nominally in force till 1666, exactly a century after the passing of the first. But in the meantime antimony was gaining ground, quickened, no doubt, into a kind of popular fictitious reputation by the opposition of the doctors, till at last both Cardinal Mazarin and the King had been submitted to treatment by it at the hands of Guénaut, and after a controversy altogether unexampled in its fierceness and extravagance upon both sides, the balance in the Faculty itself began to incline in favour of antimony. Even so late as 1651, however, Guénaut, Beda, and Cornuti were formally condemned by the Faculty for being led away by antimony, "*spe lucri,*" writes Patin confidentially; and this sentence, he adds in high jubilee of triumph, "has made them return to their duty, and if hereafter they fail in it we will not fail them; we will apply the law and the power of the decree so keenly that they will remain expelled by it. . . . If we had suffered these three there, they would no doubt have attracted others, by the rule *Abyssus Abyssum invocat.*"[2] But, alas! for big words and high-handed judgments! In 1666, when Patin was no longer Dean, having been replaced by Le

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[1] Facultatis de Antimonio Censura. Decretum in Scholis Medicinæ, tertio Calend. August, anno 1566.

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[1] This, with the preceding, I have taken from a foot-note in the edition of Gui Patin, edited by Reveillé-Parise, vol. i., p. 191. The Latin text is, however, extremely ill printed, and barely intelligible as there given.

[2] Op. Cit., vol. ii., p. 587.

Vignon, the Faculty of Medicine of Paris again met on the 29th March, and "in an assembly of one hundred and two, decided by the vote and suffrage of ninety-two doctors, that *antimony is to be numbered among purging medicines, and that it shall be in the power of every doctor to prescribe it,* on fitting occasions, and to write and dispute publicly about it, provided always that these things are done for the general good." This decree was also confirmed, like the former ones, by an edict of the Parliament of Paris; which, however, added a prohibition to all persons against the use of this medicine without the advice of the Faculty.[1]

One may imagine the disgust of Gui Patin at being thus foiled with his own weapons, and seeing his beloved Faculty invaded in force by the "*troupe stibiale et stigiale,*"[2] as he calls them in his somewhat savage irony. "I console myself," he writes, "because there must be heresies, in order that the good may be tried; but I have never been of the humour to adore the golden calf, nor to consider fortune as a goddess. . . . When the wind shall have changed, all these champions of antimony will be dispersed like the smoke of their furnace." And so he withdraws quietly from the controversy. We hear little more either of Patin or of the much-abused Guénaut thereafter in the Faculty of Paris. Guénaut, indeed, died the next year of an apoplexy, as we learn in a brief paragraph of a letter under date 17th May, 1667. It is curious that not a trace remains of the personal history or of the professional labours of this great disturber of orthodoxy in the seventeenth century. Boileau mentions him twice, in neither place in a complimentary manner. He is supposed to have furnished forth the dramatic portrait of M. Macroton, by Molière in the *Amour Médecin.* It may be, perhaps, fairly presumed that the reproaches of the old Dean of Faculty were not altogether undeserved, and that this arch-antimonialist was one of those nimble and shifty persons who study much more effectually the ways of putting money in the purse, than of adding to the real knowledge and resources of the art of medicine.

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[1] Patin's letters, *ut supra*, vol. iii., p. 609, gives the decree of the Parliament; that of the Faculty I have found in Raynaud, *Les Médecins au temps de Molière*, 1862, p. 212.

[2] *Lettres de Gui Patin*, vol. ii., p. 96.

THE LESSON OF THE PAST.

It is, perhaps, worth while to have detained you thus far over this old story, in order to draw from it the moral which it supplies. The century of bitter and violent controversy here alluded to did almost nothing, after all, towards the settlement of the question. The actual therapeutic properties of antimony were scarcely at all brought into view amid the dust and noise of the strife, and although pamphlet followed pamphlet in endless succession, filled with all the extravagance of the language of intemperate discussion, there is absolutely not one work of the whole farrago that contains a fact known to the nineteenth century, or is referred to as in any degree valuable in the numerous investigations which the last hundred years have produced as regards this powerful and yet dangerous remedy. Its powers for good and for evil were alike lost sight of in the confusion of the struggle, and instead of a careful and judicial inquiry we have the humiliating spectacle presented to us of a mere wrangling for power, under the watchwords created by a statutory orthodoxy on the one side, and a fashionable heresy on the other. On the whole, it may be said that, had antimony never been discovered by Basil Valentine, the doctors of the Paris Faculty would have been none the worse, and their patients surely much the better. To arrive with anything like safety at an estimate of the value of this remedy, or a knowledge of its true properties, required not only a different temper of mind, but a far more enlarged circle of ideas, and a more carefully educated experience than that of any one of these old physicians.

How far have we that improved temper, that enlarged circle of ideas, that more carefully cultivated experience? How far can we hope to apply these to the ever new and unsolved difficulties of our own daily life and duty? How far does the instruction supplied to you, or placed within your reach, in this school, enable you to rise out of the mere routine of dead orthodoxies, and to take your place among the highly-endowed minds that are now everywhere working in the impartial light of modern science, at the problems of practical medicine?

These are questions, gentlemen, of no small importance to all of us. But more especially are they of importance to you, with whom the door is not yet closed, by the harassing and absorbing practical duties of life, to the elementary instruction which is to fit you for these duties. Suffer me therefore to address to you a few considerations on this subject, and to keep in view in doing so that you are not only pupils of a medical school, but members of an ancient University.

The first lesson to be learned in order to make all other lessons possible is, in my opinion, this—to deal very largely with things, and not with mere words; to realize, as much as you can, all your instruction, by making it your own through personal observation; to suffer nothing, if it can possibly be avoided, to lie in the mind as a dead weight of vocables, oppressing the memory, and dwarfing the intellect; but to bring everything into the living light of fact and of nature, and thereby at once assure to yourself the truth and exactness of your knowledge, while at the same time you are stamping it down upon the memory by the most sure and lasting of all technical methods.

This is essentially the modern spirit of scientific inquiry, in virtue of which alone you can rise out of the dogmatisms and orthodoxies of the past, to make secure and beneficial progress in the knowledge of your profession. It is to imbue you thoroughly with this spirit that we give you the freedom of dissecting-rooms, and laboratories, and museums, and hospitals. It is for this reason also that we urge upon you, as a study of the first importance to your future welfare and progress, the fascinating pursuit of natural history, whether in the realm of geology, or botany, or zoology. That study, when rightly pursued, is, I hesitate not to say, at once the most agreeable and the most effective discipline for the work of the dissecting-room, the laboratory, or the hospital. It trains at once the mind and the senses; and through the most wholesome and delightful observations of detail it leads up to the most profound generalizations and the most far-reaching theories. In even so simple a set of objects as those which lie between the petals of a daisy there are whole depths of thought, a thousand times more practical and more useful to you as medical men than all the speculations and assertions of all the pseudo-philosophers who wrote for or against antimony, from Basil Valentine in the thirteenth century to Gui Patin and Guénaut in the seventeenth. For in the one case you are feeding your mind with substantial truth, and realizing it step by step as you go along; in the other you are feeding on the wind of doctrine, and the result is simply nothing or worse than nothing—wasted time, disappointed effort, a painful sense of utter vacancy—"Vanity of vanities," saith the preacher, "all is vanity!"

In the Middle Ages, and to a considerable extent also, though not without exceptions, in the early ages of the Greek philosophy, the whole method of science was precisely the opposite of that which I commended to you in the study of the daisy. It was to place before the mind in the first instance the most abstract propositions and ideas, and to reason outwards *towards* the facts. Very often the facts were never reached at all, and then the whole process of thought ended, in what?—of necessity in formulas of words, which were handed down from teacher to scholar as ultimate solutions of the problem. Thus Democritus reduced the principles of things to a "concourse of atoms," and was followed by the Epicureans, and by Lucretius; but neither Democritus nor the Epicureans, though setting forth a purely physical philosophy, concerned themselves in the least with the exact observation of facts, except in so far as was absolutely indispensable to give something like body and substantial illustration to their purely hypothetical atomic theory. So, too, the great controversy of the twelfth century was as to the essential *reality,* or the merely *nominal* existence, of genus and species, property and accident; but no one thought for a moment of extending thereby the knowledge of the actual *objects* included under genera and species, or of the actual *qualities of things* included under the description of properties and accidents. To do this would have appeared to Abelard, or to the followers of Aquinas or Duns Scotus in the two succeeding centuries, not only a waste of time, but a most shameful prostitution of Divine philosophy. And so in medicine, too, the study of words and notions of the mind came in place of the observation of symptoms; while occult qualities and inscrutable masses of polypharmacy came to represent the treatment of disease.

It would extend this lecture far beyond reasonable limits were I to attempt to show you how the despotism of this false method was gradually relaxed, and the study of external objects substituted for that of mere notions or conceptions of the mind. Roger Bacon, even in the thirteenth century, opened the way in Natural Philosophy with surprising power and originality; but it was a "voice crying in the wilderness." Vesalius, of Brussels, in the sixteenth century, laid the foundations of modern anatomy, and was followed by a brilliant succession of discoverers, who, by their united labours, cleared the way for our English Harvey. Conrad Gesner, in Switzerland, explored the realm of Natural History; Rembert Dodoens, Fuchs, and in England, Gerarde, the special province of botany. Greatest of all, Galileo Galilei dared for the first time to set forth physical facts in astronomy which were not only the result of independent observation, but in direct opposition to the dicta of the Church, and thus he set free for ever (notwithstanding his own submission) a host of the most enlightened minds from the sway of authority in matters of science.

But meantime the study of medicine proper, that is of the healing art, remains even now to a very large extent governed by precedents derived from the past. Nor can this ever be otherwise; for the whole art of medicine is steeped in antiquity, and draws a large part of its most accurate and striking observations from a date long before the Christian era. So long as it can be said with truth that we inherit the labours and the very language of the schools of Cos and Cnidus—so long as we follow prognostics derived from Hippocrates, and details of symptoms taken from Aretaeus, it cannot be a matter of indifference to an enlightened medical man, whether or not he knows those languages which bridge over the chasm between the nineteenth century and those remote epochs. I am not at all, therefore, on the side of those who, because they have cast aside the fetters imposed on our art in the Middle Ages, would therefore forget that medicine has a history. Rather would I have you use the freedom of the modern physician in such a way as to make it a graceful and real tribute to those great men whose names still shed a lustre over the healing art, and over the history of human opinion in all the arts and sciences.

XII.—DR. ALISON.

[THE following tribute to the memory of one who was regarded by very many of his pupils in all parts of the world as "a valued teacher, a true friend, a great and good man"—was published, shortly after Dr. Alison's death (22nd September, 1859), in the *Edinburgh Medical Journal* for November, vol. v., part 1, p. 469. Although undertaken at a moment's notice, and under pressure of circumstances, it contains so much of the essentials of a biography as may probably make it acceptable, even now, to some readers of the present volume; and although I cannot absolutely affirm that my more mature judgment is in accordance with everything that is here stated, I think it better to reproduce this brief memoir without any alteration, except the omission of an introductory paragraph. No other and more extended life of Dr. Alison having ever been given to the public, it may be as well to add that the whole of the information as to facts in the present sketch was either received from, or submitted to the revision of, his surviving relatives, who were kind enough to approve of the publication.]

WILLIAM PULTENEY ALISON was born in 1790, at Boroughmuirhead, near Edinburgh. His father, the

Rev. Archibald Alison, was also a native of Edinburgh, and the son of a magistrate of the burgh. His mother was a daughter of Dr. John Gregory, and sister to Dr. James Gregory, two eminent members of a family distinguished in letters and science, as well as in the history of the medical school and University of Edinburgh. William, the eldest of the family (Sir Archibald Alison, the eminent historian, being a younger brother), spent his earliest years at Kenley, in Shropshire, of which his father was perpetual curate. He received the elements of education from the parish schoolmaster there, under the superintendence of his father, who also instructed him in Latin till after his removal to Edinburgh in 1800, when he was placed under the care of Mr. Dunbar (afterwards Professor of Greek), and Mr. Craig (afterwards minister of Jedburgh), as private tutors in classics. In 1803, at the early age of thirteen, he was sent to Edinburgh College, and remained there steadily engaged in the pursuit of letters and of medicine till 1811. Though never at a public school, there is little doubt that his early education must have been an object of no common solicitude to his father, who was a man of great literary accomplishments, and had already given to the world the first edition of the "Essay on Taste," which afterwards attracted much attention as the basis of a well-known article in the *Edinburgh Review*, in which Jeffrey expounded a theory of beauty now generally regarded as heterodox, but at that time (1811) commanding a large amount of admiration and support.

The Rev. Archibald Alison and his family left Kenley in 1800 for Edinburgh, where he was called to the senior charge of the Episcopal congregation, which then assembled in the Cowgate Chapel. His amiable character and refined eloquence raised him to a very high rank as a preacher, and the obscure chapel in the Cowgate was soon exchanged for the more modern and more ambitious structure of St. Paul's in York Place. The house of the popular clergyman became the centre of a select social circle, of which Dugald Stewart, James Gregory, Lord Woodhouselee, Telford the engineer, Playfair, Jeffrey, and Francis Horner, were among the most intimate members, while Henry Mackenzie (the "Man of Feeling"), and Sir Walter Scott, with almost all the literary celebrities who either resided in Edinburgh or visited it for a season, were occasional visitors. Many of the friendships thus formed by young William Alison were only broken by death. Telford continued, during his life, to spend several days with him every year. The intimacy of his relations with the family at Woodhouselee is shown by many most affectionate and familiar references in the Memoir lately published of the late Patrick Fraser Tytler, author of the *History of Scotland.* For his uncle, Dr. James Gregory, and for Professor Dugald Stewart, he seems to have had an almost filial regard;

and there can be little doubt that the fine moral enthusiasm of the latter, by which, as Mackintosh declared, "he breathed the love of virtue into the souls of thousands of students," was calculated to engage all the best affections of the young votary of literature and medicine. No lectures, accordingly, made such an impression on him as those of Dugald Stewart, which were, indeed, at that time the glory of the University of Edinburgh in the department of general education. The tendency towards abstract thought, thus early implanted, kept hold upon him throughout his life, and though often subordinated to the more immediately practical and useful occupations of his prime, expanded again into full and unchecked activity towards the close of his career, after his retirement from active business. We have been informed that Dugald Stewart thought so favourably of him as to have at one time entertained the earnest desire that his young friend should succeed him in his Chair. That there continued to be a strong intellectual sympathy between them long after this design must have been abandoned is shown by an article which, so late as 1817, Dr. Alison contributed to *Blackwood's Magazine*, and the object of which was to vindicate Stewart's philosophy from the criticisms of the *Quarterly Review*.

Another ruling passion of the young man was one which seems strange to those who knew him afterwards only in his professional relations. If ever there was "a man of peace" in all personal matters, it was Dr. Alison; yet we are credibly informed that "his early predilection was for a military life, and that it was only in deference to the urgent wishes of his father that he abandoned his intention of entering the army. Yet through life he continued to take a deep interest in everything pertaining to military affairs, was fond of perusing details of campaigns and battles, and watched with interest the progress of the various wars which have happened in our own times."

That a heroic and essentially chivalrous nature like that of Dr. Alison should, notwithstanding his remarkable gentleness of disposition, feel attracted towards the profession of arms at a time when all Europe was ringing with the news of battle, need not surprise a generation which has seen so many instances of gentle humanity combined with every soldierly virtue. Yet we cannot bring ourselves to regret that the arts of peace prevailed with him over those of war, and that the influence of his father, and probably of his uncle, Dr. Gregory, fixed his attention on that noble profession, which enabled him to win so many bloodless victories over physical and moral evil.

After graduating in 1811, on which occasion he wrote a thesis on the highly characteristic subject, "De Viribus Naturæ Medicatricibus," Dr. Alison spent a short time at home in occupations of which we have no very special account. In 1814 he made a short tour in France, Holland, and Flanders; he witnessed the entrance of the Allied Armies into Paris, and contributed a sketch of his observations to a small volume of "Travels in France," published in Edinburgh by his brother, in conjunction with Mr. Tytler, in the succeeding year.[1] The year 1815 found for him a congenial field of labour. The New Town Dispensary originated in the latter part of that year, and Dr. Alison was from the first one of its most energetic medical officers. The new principle of home-visitation of the sick poor, introduced into that institution, was enthusiastically carried out by the medical officers, and was highly appreciated by the objects of the charity; and the extraordinary popularity of the Dispensary, no less than the zeal of the medical men, was demonstrated by the fact, stated in its first annual report, that during the eighteen months succeeding its opening the names of 8062 persons were entered on its books, and of these, 2401 were attended at their own homes. As the majority of these cases must have been medical, and as Dr. Alison was at this time one of only two acting physicians (the other being the late Dr. J. H. Davidson), it is very plain that the metaphysical student and medical graduate of five-and-twenty had not allowed either the charms of Dugald Stewart's eloquence, or the "pomp and circumstance of glorious war," to impair his capacity for honest, hard, homely work. He was from this time onward the very slave of his profession, and laid the foundation of his success and reputation in a daily drudgery as unremitting, and performed with as little care of immediate reward, as if he had been working in a sugar plantation under the eye of a task-master. And no doubt he had learned thus early that "the great Task-master's eye" was truly upon him; that his path in life was marked out for him by a higher hand than his own; that not as a penance, but as a labour of love, he was charged with the mission of ministering to the poor, till he had fathomed the mysteries of destitution, and seen with his own eyes the causes of the physical degradation and moral depravity which lay at the root of so much disease and misery as he was called on to relieve.

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[1] We are informed that on two other occasions he passed a few months on the Continent, viz., in 1814, when "he ascended Mont Blanc, and was stopped by a tornado of wind and rain, within an hour's walk of the summit; and in autumn 1825, when he made the tour of Monte Rosa, and visited Milan and Turin." On one or other of these occasions, or perhaps both, he must also have visited the Bernese Alps, as he speaks in one of his works on the poor-laws, of having particularly inquired into the character of the Bernese peasantry in the Grumanthal, the most pauperised district of that canton, with which he describes himself as familiar from personal observation. He had no doubt availed himself of these opportunities for procuring information as to the working of the poor-laws on the Continent generally.

The idea of a close connection between epidemic fever and destitution seems to have very early pressed itself upon the attention of Dr. Alison, but to have developed itself into a practical shape more gradually, as his ideas of social economy became more matured. From the quarterly medical reports of the New Town Dispensary, published in the *Edinburgh Medical and Surgical Journal* for 1817-19 (vols. 13-15), which were in great part written by Dr. Alison, it would appear that the epidemic fever of these years was not at this time decidedly referred by him to the destitution which had preceded it, as one of its well-marked predisposing causes. It may, perhaps, be fairly inferred that it was subsequent to this period that the way was cleared in his mind for that idea, of which he made such powerful use afterwards, that fever, as observed in Ireland and Scotland, was due to causes in a great measure removable by legislation. The contagious propagation of this epidemic, however—its partial distribution; the insufficiency of *mere* dirt, or poverty, or other causes in constant operation, for the generation of a specific poison; the origin of fever cases in particular localities, from the association of the sick with the healthy in confined apartments; and the measures of prevention, now so well understood, but unhappily still so imperfectly practised, resulting from these principles,—are all laid down in these papers with a decision and a clearness of illustration which leaves nothing to be desired, and which could only have sprung from profound personal conviction, based upon a most intimate and thorough knowledge of details.

It is now well known, through the very clear expositions of Dr. Christison, and also from an examination of the contemporaneous records of the fever of 1817-19, that a great proportion of this epidemic consisted of that type which has since been recognized under the name of "relapsing fever," and thereby distinguished from typhus. The reports of the New Town Dispensary leave no doubt that at the time to which they refer, as indeed for long afterwards, Dr. Alison did not see in the fevers of Edinburgh the evidence of more than a single specific disease, appearing under different forms. The manner in which he greeted the "relapsing fever" of 1843 as a "nova febris" (to use the expression of Sydenham), the revolution it created in his ideas on the subject of the unity of the fever poison, and the faithfulness and detail with which he worked out to his own satisfaction the evidence of two co-existent types of febrile disease, must be fresh in the memory of many of our readers. The fact that we now know he had seen this fever before, and seen it through the distorting medium of an erroneous theory, serves only to illustrate the remarkable freedom from prejudice with which Dr. Alison always entered on the discussion of new questions when fairly presented to him. He was, as his antagonists in argument knew well, not at all inclined to that soft-headed scepticism which does not entertain decided opinions at all, for fear of having to modify them. For a quarter of a century he heartily acquiesced in and strenuously maintained the current doctrine of the schools upon this subject; but the moment that new evidence was presented to him, he mastered it, observed for himself, re-formed his opinions, and became one of the most decided adherents of the new theory.

Two other observations may be made, in passing, upon these interesting reports, as bearing relation to controversies that have subsequently occupied the attention of the public. The first is, that the occurrence of an eruption "bearing a very near resemblance in colour and in figure to the advanced stage of the measles" was observed "in a few cases" only in the fever of 1817-19, although petechiæ appear to have been carefully looked for from the first.[1] In other words, a truly *eruptive* typhus proved only a small part of this epidemic; an observation entirely accordant with the observations of Dr. Christison as to the rarity of "true typical typhus" in 1817-19.

The other conclusion which may be drawn from this record of practical experience is, that Dr. Alison, who appeared before the public on a much later occasion as the warm advocate of the efficacy of blood-letting in certain forms of disease, was at this time using all his influence to restrain the use of the lancet in the "synocha" (as Dr. Christison terms it) of 1817. He records that "the old practice of bleeding in continued fever has again become fashionable within these few years." He has tried it, and seen it tried; he thinks it may relieve particular symptoms; but on a fair comparison of the cases in which it was employed with those in which it was omitted, he "cannot say that it seemed to shorten the disease." Further, he is of opinion that bleeding has no tendency, as has been asserted, either to restrain or to favour the occurrence of typhoid symptoms; and thinks that "those who were treated merely by the cool regimen, sponging the skin, clearing the *primæ viæ* upwards and downwards, in the commencement, and keeping the bowels open in the course of the disease, remained equally free from these symptoms in at least as many cases." The curious statements of Dr. Christison, and the work of Dr. Welsh,[1] will show how much these cautions were needed, as well as how little they were attended to at the time. The dogmatists of 1819 were very confident that the fever of that year was like all other fevers; they utterly disbelieved the possibility of a change of type in disease; and setting aside with contempt the opinions of Cullen and others as unworthy of confidence, they referred the "supposed diversity" of what was called typhus fever "rather to the *mental* revolutions of practitioners than to the actual revolution of disease." And thus they arrived at the conclusion that "the practice or neglect of general blood-letting is the main cause of the absence or presence of a typhoid character in different epidemics."

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[1] Compare pp. 249 and 401, *Edin. Med. and Surgical Journal*, vol. xiii.

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[1] *Welsh on Blood-letting in Fever*, p. 43.

Another subject which occupied Dr. Alison's attention at this time was the occurrence of cases of a disease resembling chicken-pox in persons who had been properly vaccinated, but in such suspicious connection with cases of small-pox in the unvaccinated, as to suggest the idea of their being in all probability modified forms of that disease. The occurrence of modified small-pox after vaccination was indeed admitted by the immediate followers of Jenner, as well as by William Moore, Bateman, and others; but it was with great unwillingness that the admission was made as regards cases in which vaccination had been perfectly performed. The experience of Dr. Adam in Forfar, and that of Dr. Dewar at Cupar, had, however, clearly established the *epidemic* occurrence of a modified small-pox; and many interesting questions had accordingly arisen as to the dependence of this affection upon imperfect vaccination, or upon the length of time which had elapsed since this protective operation had been performed. It had also been mooted as another solution of the question, that the cases regarded as modified small-pox might in fact be "an aggravated variety of chicken-pox;" and it is to the diagnosis of chicken-pox and varioloid that Dr. Alison chiefly addresses himself. It is curious that while he was thus revolving in his mind whether the cases regarded as modified small-pox were not, in fact, chicken-pox, Dr. John Thomson seems to have been led by a similar class of facts to entertain the view that all the cases hitherto regarded as chicken-pox were in reality nothing but modified small-pox. Dr. Thomson defended this opinion in the well-known work on the Varioloid Disease; while Dr. Alison, after carefully examining into the facts within his own observation, was led to the conclusion, that close as was, confessedly, the resemblance in some cases, there were, in fact, two separate contagions involved in the affections termed chicken-pox and modified small-pox. The mode in which Dr. Alison arrived at this conclusion may be discovered from his review of Dr. Thomson's work,[1] and many of the facts on which his opinions were founded are given in detail in the report of the New Town Dispensary for the summer quarter of 1818.[2]

In 1820 Dr. Andrew Duncan, jun., who had held the Chair of Medical Jurisprudence from its creation in 1806, was appointed conjoint Professor of Institutes of Medicine with his father; and the vacant Chair of Medical Jurisprudence was conferred by the Crown upon Dr. Alison. He had hardly held this appointment much more than a year, when his uncle, Dr. James Gregory, died. Dr. Alison had been engaged, during the latter years of this great man, in assisting him in the delivery of the course of Practice of Physic; and he was probably more favourably known to the students in this manner, than through his brief tenure of the Chair of Medical Jurisprudence. Dr. Home's appointment to the vacant Chair of Practice of Physic in 1822 caused the removal of Dr. Duncan, jun., to that of Materia Medica; and Dr. Alison was promoted from the Professorship of Medical Jurisprudence to that of Institutes of Medicine. This Chair he occupied for a period of about twenty years, at first as the colleague of Dr. Duncan, sen., and afterwards as sole professor. His professorship likewise gave him access to the Infirmary as one of the physicians in the clinical wards.

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[1] *Edin. Med. and Surgical Journal*, vol. xvi. p. 225.

[2] *Ibid*, vol. xiv. p. 389.

The multitudinous occupations connected with his University appointments seem to have left little time for other use of the pen than in the preparation of lectures; and Dr. Alison published little until some time after the establishment of the Medico-Chirurgical Society of Edinburgh in 1821. Notwithstanding his many occupations, however, he discharged the duty of secretary to that body, along with Dr. Robert Hamilton, from the commencement, and in June, 1822, he read the first part of a valuable paper on the causes of scrofulous disease, which was afterwards published, together with a supplementary paper, in the *Transactions* of the Society. Whatever may be thought of the pathological speculations in this paper as to the connection of tubercles with inflammation—a subject which has now lost much of its apparent importance—there can be no doubt of the substantial accuracy of the view which Dr. Alison was at so much pains to establish by an elaborate induction, that the predisposing causes of tubercular disease are not to be found in climate so much as in the debilitating modes of life, and the deficient light and air operating on the health of the masses, and particularly of the children of great cities. This opinion, opposed as it was to much of the popular as well as professional prejudice of the time, is worked out with a degree of clearness which is quite extraordinary, considering the very imperfect data which statistics and sanitary science then afforded. Another paper, which Dr. Alison furnished to the *Transactions of the Medico-Chirurgical Society,* "On the Physiological Principle of Sympathy," has now chiefly a historical interest, as bearing on Sir Charles Bell's discoveries, and may be passed over in this slight sketch along with some others, contributed to the *Medico-Chirurgical Review,* some years afterwards, on Dr. Marshall Hall's views.

In 1831, Dr. Alison published his *Outlines of Physiology,* embodying the substance of his prelections on the subject. This work was afterwards extended and completed by the publication of *Outlines of Physiology and Pathology* in 1833, so as to form a complete text-book to a course of lectures on Institutes of Medicine. The amount of compressed thought in this work precludes any attempt to notice it here in detail, further than to say that it contains the basis of those profound speculations which were afterwards developed in a more extended form in his article on Inflammation, and in the papers on Vital Affinity in the *Transactions of the Royal Society of Edinburgh.*[1] The idea of a *life-force* or *forces*, of something distinct from, and superadded to, the physical forces of dead matter, was the ruling idea of all Dr. Alison's physiological speculations. These vital forces are, according to him, quite as distinct from the mind and its special endowments as from the physical forces; and yet the whole realm of life is subservient, in the plan of creation, to the manifestations of that "divinæ particula auræ"—the intelligent and sentient principle in the higher orders of the animal creation. Vegetables exist, to make animals possible; animals find their culminating point in man, in whom the immortal soul shines forth as the special image of the Divine majesty and glory. Throughout the range of this animated creation we find peculiar laws of being which may be termed *vital*, and of which organization is the result. Two modifications of vital force are especially known to us:

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[1] Vols. xvi. and xx.

one in alliance with the mechanical properties of matter, giving rise to *vital contraction* or *muscular motion;* the other, grafted upon its chemical properties, and shown forth in *vital attractions and repulsions* of the ultimate molecules. These peculiar phenomena can be studied only in living beings; there is nothing analogous to them in dead matter. Nor are they to be confounded with each other, although motion is necessarily the result of both. Vital contraction is inherent in particular tissues; vital attraction is shown forth in every part of the organism, at every moment of nutritive, secretive, absorbent change. The chemical, or rather chemico-vital changes in the ultimate molecules are, in fact, among the great motive powers of the organism. They rule the movement of the blood in the capillaries, and thereby control to a great extent the rapidity and force of the circulation; they bring about the asphyxial stasis in the vessels of the lung, and determine the greater part of the phenomena of inflammation; they produce the cohesion of the blood-globules even outside of the vessels, and, among the lower organisms a multitude of other interesting molecular movements, totally inexplicable on any other hypothesis. Such are the more salient points of what may be called Dr. Alison's physiological system; with which, too, his pathology was deeply penetrated, resting as the system did on the doctrine of inflammation as one of its main pillars.

For several years after the publication of his *Physiology and Pathology,* Dr. Alison seems to have been greatly intent on the inquiries suggested by it; and, with the exception of a dissertation on the History of Medicine in the *Cyclopædia of Practical Medicine* (published in 1834), almost all his writings have reference in some way or other to the speculations above indicated. To the *Cyclopædia of Anatomy and Physiology* he contributed several elaborate articles; for the *Library of Medicine* he wrote the chapter on Inflammation. To the British Association of Science he read, in 1835, a "Notice of experiments on the vital power of arteries leading to inflamed parts; and on the immediate cause of death by Asphyxia." His early metaphysical predilections also found scope in a paper read before the Royal Society of Edinburgh, "On single and correct vision, by double and inverted images on the Retina;" and in an article on Instinct for the *Cyclopædia of Anatomy and Physiology.* The only considerable contribution to practical medicine, strictly so called, which we can find during this period, is a series of cases of what has been called "Jaundice from non-elimination," in the *Edinburgh Medical and Surgical Journal* for 1835 (vol. xliv. p. 287).

But, notwithstanding the abstract character of studies which were to him but the recreations of his mind, Dr. Alison relaxed no whit in his pursuit of practical knowledge. He was unwearied in his attendance in the Royal Infirmary; he was more than ever the devoted and trusted friend of the poor. Among the better classes, too, his practice was extending, though it did not become really large till some years afterwards. During the visitation of the cholera in 1831-1832, he was at his post as one of the representatives of the College of Physicians at the Board of Health. And in this case, as in several fever epidemics, he had an opportunity of appreciating the great importance of early home-visitation, and the removal of the healthy, for a time, from the affected localities, by means of houses of refuge. The results of his experience upon this subject, and on the communication of cholera in general, were not laid before the public till long afterwards.

And now, towards the year 1840, amid abstract physiological discussions, and wearisome details of infirmary and dispensary practice, Dr. Alison's mind was gradually ripening towards conclusions which could no longer be suppressed. Fever had manifestly been for some time on the increase, not only in Edinburgh, but in most of the large towns of Scotland, and in every part of Ireland. The epidemic of 1827-28 was more severe than that of 1818-19, and the numbers annually affected between these two great epidemics had been constantly higher than at any former period. There was reason to think that the destitution which preceded the epidemic of 1827-28, partly as a consequence of bad crops, and partly as the more remote result of the commercial crisis of 1825, had something to do with the immense extension of fever at that time. The reports of Drs. Barker and Cheyne, moreover, published in 1821, seemed to have traced the connection between fever and famine through the long and painful story of Irish disease for more than a century. Not only was there an increase in the amount of fever in Scotland, but from 1830 onwards, Dr. Alison began to be satisfied that there was an important change in its character. Fever was becoming more fatal. Typhoid phenomena, rare in 1819, and even in 1828, had become common. Reaction was imperfect; depression and collapse came on early, unless the strength was supported by nourishment and wine; blood-letting, if attempted at all, could not be carried out, from the faintness caused by the loss of a few ounces of blood; and eruption, formerly a comparatively rare concomitant of fever, was becoming much more frequent. Between 1832 (the year of the cholera epidemic) and 1840, the annual number of fever cases in the Edinburgh Infirmary was not once under 650, while from 1800 to 1816 it had scarcely ever exceeded half that number. But in 1832 the numbers were 1394; in 1837 they were 1224; and in the two succeeding years 2244 and 1285. Such facts could not fail to strike every thoughtful mind; but with many persons of no mean intelligence they inspired only a vague feeling of dismay. To Dr. Alison they gradually brought the conviction that there was something wrong in our social organization, and that the wrong admitted of a remedy.

In 1818 the General Assembly had given in a report to the select committee on the Poor-laws. The English system of relief of the poor was at this time characterized by a most injudicious profusion; the Scottish system was, on the other hand, marked by the most rigid and scrupulous economy. Two peculiarities were further remarkable in the Scottish poor-law—the dependence on voluntary contributions in the great majority of parishes, and the firm resistance opposed by public opinion, no less than by the terms of the law, to the claims of the "occasional" poor, under the pressure of temporary disease or calamity. The General Assembly declared in favour of the Scottish system on both these points, and, in particular, declared in the strongest terms their conviction "of the pernicious tendency of compulsory taxation." Shortly after this report was given in, a period of severe distress brought hundreds of Paisley shawl-weavers to the brink of starvation, by throwing them suddenly out of employment. They applied to the parish for relief; but the heritors and kirk-session refused to comply with the application, on the ground that the applicants, being able-bodied, "did not fall within the class of poor for which the law provided." Application was then made to the sheriff, who adjudged the heritors and kirk-session to meet and assess themselves for the relief of these persons. But an appeal being made to the Court of Session, the decision of the sheriff was overruled, and his supposed right to interfere with the parochial authorities was set aside. It was thus determined judicially, that in Scotland there was practically no appeal for a labourer out of employment, and in danger of starvation, against the decision of those who were charged with the administration of the poor-law; even when, on principle, they declared him not to be a proper object for relief.

Such was the working of the poor law in Scotland in 1819 and for many years afterwards. Only such persons as were permanently disabled were recognized as having a legal claim to relief. For such only was a regular allowance set apart—this allowance being derived from the voluntary contributions at the church doors, in the majority of cases; in some parishes, however, supplemented by a small assessment. An occasional dole to a labourer struck down by sudden calamity, or disabled by want of employment to the verge of starvation, was looked upon as an extreme stretching of the letter of the law, and as a most dangerous and untoward precedent.

The law, thus interpreted, and thus acted upon, led directly and most naturally to two consequences.

Large numbers of the population were constantly living in a state of great destitution; and this destitution would, in times of difficulty, often have been much more extreme—would, in fact, have amounted to direct starvation—had it not been for the supplementary relief afforded by private charity. The country swarmed with beggars; and indiscriminate almsgiving relieved the distresses of those whom the law neglected —at the cost, however, of a large amount of money squandered on worthless objects.

Yet this state of the law was admired and defended by men of the most unquestionable probity and goodness of heart, on the ground that private charity was the only fitting supplement to the deficiencies of the public allowance; and that to give to a merely starving man a claim in law to subsistence, was to degrade and demoralize him—a plea which implied of course, and, indeed, was plainly admitted to imply, that the abuses of private charity were more tolerable than those inherent in the legal claim to aid on the part of the destitute members of society.

There was something in the state of England which gave a colouring of justice to these representations. In England there was comparatively little mendicancy, and no serious destitution. But, on the other hand, there was, what probably existed to a very slight extent indeed in Scotland, a vast misapplication, not of private, but of public money; which, collected everywhere by a compulsory assessment, was known to be often employed in the support of unworthy objects, as well as in the payment of regular wages, and occasionally, as was pretty clearly proved, in the maintenance of positive crime. A remedy was loudly demanded for these evils, and the indefinitely increasing poor's-rates; and, not unnaturally, the economy and simplicity of the Scottish system was pointed out by many as a model worthy of imitation. The result was, that a commission was appointed to inquire into the abuses of English poor-law administration, with a view to their reform; while the Scottish system, with serious, though exactly opposite faults of its own, was allowed to work out still further its peculiar results, and came, in fact, to be contemplated by its adherents with increasing complacency.

The English Poor-law Amendment Act was passed in 1834; and in 1838 the Irish Poor Relief Act, on a somewhat similar model, passed into a law. It was about this time that Dr. Alison turned aside from his strictly medical duties with the resolution to expose thoroughly to the people of Scotland the iniquities of their vaunted system, which, even so late as 1839, was again weighed in the balance of a report to the General Assembly, and pronounced to be sound in principle, if not absolutely without fault in detail. It is only fair to admit that the experience of the past had not been altogether lost upon the reporters on this occasion; for they considered "that the situation of people destitute of employment was not to be overlooked, and that many cases might occur in which men of this class ought to obtain temporary relief in times of occasional sickness or unusual calamity, although not as a matter of right." Still, the General Assembly, and almost all the persons most influential in guiding public opinion in Scotland, were persuaded, and prepared to act to the full extent upon the persuasion, that the mere existence of a poor-law, except for the purpose of warding off absolute starvation from the totally and inevitably destitute, was an evil to be avoided, as necessarily fraught with all the mischief then currently attributed to the English system.

It was against this fatal prejudice, as he considered it—a prejudice deeply rooted in the popular mind of Scotland, with its characteristic love of independence—a prejudice, too, shared by most of the landed proprietors, and embodied in repeated deliverances of the General Assembly, and of various municipal bodies—that Dr. Alison considered it his duty to make a firm, solemn, and temperate appeal. His position will hardly be accurately understood, if he is considered as merely protesting against an unjust state of the law; his object was not so much to carry the Legislature, as to move the heart of the country. He even declared that it was not, in his opinion, necessary, in the first instance, that the provisions of the English poor-law should be introduced into Scotland. "Probably," he wrote, "the law of Scotland, *if administered in the same spirit*, would answer the same purposes; but, in order that this may be, it seems absolutely essential that all who are charged with the administration of the poor-laws, in all parts of Scotland, should cease to regard them as an evil."[1] To this end Dr. Alison now applied the whole force of his mind, and all that familiar and detailed knowledge of the state of the poor which he had derived from many years constant intercourse with them in hospital, dispensary, and private practice.

In bringing a bill of indictment against his countrymen (for such was the practical effect of his writings on this subject), showing that they had grossly neglected the interests and even the legal and just claims of the poor and destitute, and had thereby allowed a large portion of the community to fall into a state of physical and moral degradation disgraceful to a civilized community, Dr. Alison was well aware of the nature of the opposition he would have to encounter. He knew that the cry of a wounded national pride would be raised from one end of the kingdom to the other, and that it would come in aid of intellectual and dogmatic preconceptions deeply rooted in the minds of all classes, and especially of the governing classes. He knew, moreover, that the Malthusian doctrines of population, on which many of these prejudices were founded, were in some points amply justified by the very facts he had to bring forward in opposition to the deductions currently founded on them. He had himself been instructed by his revered teacher Dugald Stewart with respect to the evils supposed to spring from a legal provision for the poor by assessment; and had been accustomed to hear many of his most respected friends and associates insist on these theoretical evils in much stronger terms than Dugald Stewart. "Even independently of the authority of such men," he writes, with one of those quiet touches of gentle sarcasm which were his nearest approaches to humour on the one hand, or to offence on the other, "I am aware that almost every Scotchman, who has 'made up his fagot of opinions,' has included in it a general belief (in many instances, I believe, perfectly well-founded) that the civil institutions of his own country are superior to those of England, and on that account will be unwilling to relinquish the idea, that the Scotch principle is the right system of national charity, and the English the wrong one." "But," he continued, "Magna est veritas et prævalebit." It was no mere sentimental grievance that he had to bring before his countrymen, and he took care to appeal to them in no unmanly tone of flattery, even towards their most cherished national prejudices. While carefully abstaining from attacks upon individuals, and in all cases inclining to a generous estimate of motives in his opponents, he took care to deprive them of every advantage which they might have hoped to gain by reposing upon erroneous general principles, and refusing to look facts in the face. He boldly asserted that legal assessment for the relief of the poor was not only *not* an evil, but an absolute necessity of every highly civilized country; that instead of tending to increase the pressure of population on the means of subsistence, it was the one thing absolutely and above all other things needful for restraining that pressure; and that it did so, not in virtue of its being cut down to a minimum, or maintained at an utterly inadequate level for bare subsistence, but in virtue of its being applied at once liberally and judiciously, with proper safeguards against abuse, so as unequivocally to raise the standard of comfort among the destitute classes; thereby improving their habits, and generating those impulses of self-control which are never wholly wanting where the poor are properly cared for, but always where they are neglected and allowed to suffer extreme privations. The proof of these assertions, so diametrically opposed to the current philosophy and practice of parochial relief in Scotland, Dr. Alison drew from a comparison between the state of the destitute classes in the unassessed and in the assessed parishes of Scotland, and also from a consideration of the contrasted cases of England and Ireland. He showed conclusively that notwithstanding all the unquestionable errors of administration of the English poor-law, its working had been attended by a well-marked *and permanent* improvement in the state of the destitute classes in that country; that mendicancy, formerly as crying an evil as in Scotland and even Ireland, had been very greatly diminished, and in most places suppressed; and that private charity, so far from being dried up, was more active and discriminating in its operation than in any country in the world. Contrasting the state of England with that of Scotland, he showed that in the latter country " the higher classes do much less for the relief of poverty, and of the suffering resulting from it, than those of any other country in Europe which is really well-regulated; and much less than experience shows to be necessary in any long-inhabited and fully-peopled country, in order that the lower ranks may be maintained in tolerable comfort, and a proper foundation laid down for their religious and moral improvement." As a consequence of this neglect, he showed that pauperism or destitution worse than pauperism, which demanded relief and failed to obtain it, was not only much greater in Scotland than in other European countries similarly situated, but that it was greatly increasing; and that this increase, together with the influx of rural and Irish pauperism into our great towns, had brought them into a condition greatly more favourable than they had ever been before for the spread of epidemic disease, and had accordingly raised their mortality far above the level of corresponding towns in England or on the Continent Further, he showed that mendicancy, so far from being diminished or suppressed, was encouraged by the Scotch system; that it was, in fact, regarded as the necessary supplement of inadequate parochial relief; and that the support of the destitute population, denied by the law, and evaded by many of the rich, was thus often shifted to the shoulders of the charitable among the poorer classes, and was, besides, very inefficiently and irregularly performed, with great waste of money on improper objects.

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[1] *Observations on the Management of the Poor in Scotland, and its Effects on the Health of the Great Towns.* Edinburgh, 1840.

Such were the doctrines which Dr. Alison maintained in the *Observations on the Management of the Poor in Scotland*, with a force of logic, and an irresistible fertility of illustration, which very soon won the ear of the nation, and made the attempt to preserve the existing state of things henceforth an impossibility. It is difficult, within the limits assigned to us, to give even the faintest idea of the controversy which followed the publication of this work, or of the manner in which arguments, at once so humane and so logical, so bold in theory, and so evidently founded on large personal experience, were received by the supporters of the existing order of things. It may be said, in general terms, that even the powerful pleading of Dr. Chalmers served, in fact, only to display the weakness of the cause. The prodigious exertions by which that eminent and excellent man had been enabled to display the full power for good of the parochial system of voluntary contributions in the parish of St. John's, in Glasgow, only made it more clear than ever, that to advocate the sufficiency of such a system for the nation at large was a chimera; for in no other parish in Scotland had he been able to prevail upon the authorities to carry out his plans according to the example in St. John's; and even in that parish the attempt was, after a time, discontinued. Dr. Alison's appeal to experience, therefore, gained immensely in force, from the acknowledged failure of the parochial system; and the call for an inquiry, conducted by public authority, with a view to a reform, became irresistible. An association was formed for the purpose of urging this step upon the Government; and although this demand experienced a great deal of opposition, it became evident to all impartial observers that the existing order of things was doomed. A Royal Commission was appointed, which gave in its report in 1844; and in 1845 the conclusions of the Commission were embodied in an Act of Parliament, which, although not all that Dr. Alison had by this time come to consider as necessary, contained the germ of a great improvement, and has been worked, under the direction of the Board of Supervision then appointed, in such a way as to carry out, gradually, many most important reforms which the Act left unaccomplished. The tone of public opinion has, in like manner, undergone an alteration entirely in accordance with Dr. Alison's views, and may now be said to be in advance of the law on the subject. A great change in the mode of administration of the law is especially to be observed in regard to the relief of the able-bodied poor, when rendered destitute by want of employment. The Act of 1845 was, on this point, framed with studious conservatism, and not only abstained from disturbing the distinguishing principle of the Scottish Poor-law administration, but in express terms debarred the able-bodied poor from claiming legal relief. Nevertheless, on the occasion of emergencies occurring through the failure of the potato and other causes in 1848, it was suddenly discovered that the Act, while recognizing no claim, nevertheless *permitted*, at the discretion of the parochial boards, the application of funds raised by assessment to the relief of the able-bodied; and through the representations of the Board of Supervision, such relief has, in fact, been repeatedly granted, even on a large scale, and in a manner which entirely establishes the precedent. And further, public opinion has so thoroughly sanctioned the proceeding, that it will undoubtedly be difficult in future for parochial boards to incur the moral responsibility of refusing relief in such cases of urgency.

The discussion of this subject in greater detail would lead us too far from our present object, which is not to examine into the working of the Scottish Poor-law, but to show how deeply the spirit of Dr. Alison's suggestions was in accordance with the reforms which have been brought about by subsequent experience. And although the course of legislation has been less rapidly progressive than his impatience for good led him at one time to consider necessary, he lived to see that revolution in public opinion towards which his first efforts were mainly directed, guiding and moulding the law to ends hardly within the intention of its framers.

The publications of Dr. Alison on the subject of the poor-laws were very numerous. It is not within the compass of this Memoir to consider them separately. They are all marked distinctly by the same characteristics; over-powering earnestness, never degenerating into sentimentalism, and hardly rising into finished eloquence, but everywhere accompanied by a grave sense of responsibility, and warmed by the glow of a sustained and humane sympathy with suffering and distress. "I address myself to those," he says in one place, "who have been accustomed to look upon the poor, not as objects of disgust or aversion, but as brothers and sisters in affliction, who are born to the same hopes as themselves, look up to the same Father in heaven, and trust to the mediation of the same Redeemer; to those who remember that charity is the highest of Christian duties, and that 'our Saviour

Himself chose to be a beggar, that we, for His sake, might not despise the poor.' " And again, in speaking of the vices of the poor, so much insisted on as an obstruction to charity, he remarks, " If we reserve our charity until we meet with human beings exempt from sinful propensities or indulgences on whom to bestow it, we may reserve it for the next world, for assuredly we shall not find fit objects for it in this. The vices of the poor are generally more prominent and offensive than those of the rich, but they are very often balanced by qualities which may even excite our admiration. The very same persons who disgust us by occasional, even by habitual intemperance, we shall often find, when called on for active exertion, to be faithful and brave; when merely deprived of their liberty, to be orderly, industrious, and religiously disposed; or when afflicted with disease, to be grateful for sympathy, patient under errors and privations, candid in the admission of errors, and calm on the approach of death." We undertake to say that there is no member of the medical profession, practising habitually among the poor, who will not bear hearty testimony to the truth, as well as to the humanity, of these observations.

That Dr. Alison may have acquired a habit of greater profusion in his private charities than was either just to himself or beneficial as regards the objects of it, may be admitted without prejudice to the sound-ness of his theoretical views on the subject. It was the nature of the man to be sensitively alive to the cry of distress; and it was perhaps the one great defect of his character—the failing that "leaned to virtue's side"—that he was constitutionally unable to resist importunity, or to do anything, indeed, which implied harshness towards individuals, even in the way of duty. He was in a position which exposed him to clamorous demands on all sides, and his intimate knowledge of the justice of some of these demands made it impossible for him to put them aside without investigation, while, as regards the majority, accurate investigation was simply impossible; it is, therefore, extremely probable that a good deal of his munificence was misapplied in detail. But it was known to him, far more accurately than to most men, how much unrelieved destitution existed; and we can hardly be surprised that the refusal of charity, even to undeserving applicants, cost him, under these circumstances, a pang which was often too great to be borne. The subject is evidently one of great difficulty, on which he would have been the last to endeavour to lay down the law to others; and we feel the necessity of extreme diffidence in judging of his principles of action, though we cannot help suspecting that the tenderness of his heart often caused him to bestow alms, where reflection would have led him to believe that it was better to withhold them. His donations to public charities were less subject to objection, and were bestowed on all benevolent objects without respect to sect or party.

In consequence of the death of Dr. Home in 1842, Dr. Alison was promoted to the Chair of Practice of Medicine from that of the Institutes, which he had occupied for no less than twenty years. The promotion was a fitting one, and was conferred in a fitting manner, by the unanimous vote of the Town Council, and without solicitation or interest. In the following year he published the first part of his *Outlines of Pathology and Practice of Medicine,* which was completed in 1844. The condensed and thoughtful summary of facts and principles contained in this work made it of the greatest value to his students, to whom it conveyed an accurate impression of the more detailed instructions of the class, imbued with all that was characteristic in the habits of thought of the man. But among those who knew him not, it never attained great popularity as a text-book; nor was it perhaps intended to do so.

Nothing could exceed Dr. Alison's mental activity and bodily energy about this time. Not only was he engaged in a vast amount of literary work (being in the full flush of controversy on the poor-law question), in addition to the necessary preparations for his lectures; but his hospital duties were performed with the most exemplary regularity, the patients being often visited several times a day; while his private practice had very much increased, and now consisted to a considerable extent of consultations. The great epidemic fever of 1843, to which we have already alluded above as probably similar in many of its characters to that of 1819, was watched by Dr. Alison with the greatest interest, not only as what he then regarded as a "nova febris," but as a renewed illustration of his doctrines with respect to the influence of destitution in causing the extension of fever. He immediately wrote his *Observations on the Epidemic Fever of* 1843 *in Scotland, and its connection with the Destitute Condition of the Poor*, which was laid before the Royal Commission on the Poor-laws in October 1843, and published, with considerable additions, in May, 1844. In the same year he finished his text-book on *Practice of Physic*, and published a considerable further treatise on the poor-law question, in the form of "Remarks" on the Report of the Commissioners. He was also engaged in inquiries on the best mode of registering deaths, with a view to an Act of Registration of Births, Deaths, and Marriages in Scotland,—an inquiry which involved him in some correspondence with the Registrar-General of England and with Dr. Farr. Two years afterwards the famine of 1846 powerfully arrested his attention. The deficiency of the potato crop, in two successive years, produced alarming destitution of food in Ireland and in the remoter parts of Scotland, and great efforts of public and private beneficence were necessary to prevent the occurrence of many deaths by starvation in large districts of country. Dr. Alison thought the occasion a fitting one for bringing before the public the immense advantages secured to Ireland, in the day of her distress, by the new poor-law introduced in 1838, and the comparative disadvantage at which the Highlands of Scotland were placed in the administration of relief by the want of a law extending to the destitute able-bodied poor. We have already indicated that it was this scarcity which produced, under the direction of the Board of Supervision, an important practical change in the administration of the law in this respect; a change, however, which was only very partially brought into operation in 1848, after an extensive epidemic of scurvy and another of fever had followed in the track of famine in Ireland and some parts of Scotland.

And now, when much of his work was done, but much, to his own view, still remained to be done; when he stood by common consent at the head of his profession in Scotland, and was at the height of his reputation all over the world as a high-minded physician and generous philanthropist, came the first mysterious shock of that disease which, thirteen years afterwards, brought his life to a close. In 1846 Dr. Alison was seized with epilepsy, from the liability to which he was never afterwards quite free. He was far from being seriously disabled; the attacks were few and distant; but they sufficed to impose a check upon that tremendous energy which had been in constant operation since 1840, and which was undoubtedly injurious to his health. There was no intermission in the assiduity of his application to his ordinary duties; but his contributions to poor-law reform were after this both fewer and shorter. He continued to enjoy a large share of the public favour in practice, and was, in the highest degree, admired and respected by his students. The disease, however, continued, notwithstanding every precaution, to make slow but sure inroads on his health. He became thinner, and his memory was at times defective. The fits became gradually more frequent, and in the winter of 1855-6, after two or three attacks had occurred in the class-room, Dr. Alison felt that the active work of his life was over. He resigned the Chair of Practice of Physic in 1856, having served as Professor in the University of Edinburgh during the long period of thirty-six years.

The remainder of his life was spent in partial retirement; and during the summer he resided almost entirely at his pleasant country-house of Woodville, near Colinton. But although, as a matter of duty, he took a less active part than formerly in professional affairs, he never ceased to feel a most lively interest in all his former pursuits. On several occasions he attended the meetings of the Royal College of Physicians and took part in warm debates on medical reform. In the beginning of 1857 he attended a meeting of the Medico-Chirurgical Society, where he maintained, both by tongue and pen, his views on blood-letting and on changes of type in disease, in opposition to Dr. Bennett. He also read an address to the Royal Medical Society; and, a little more than a year before his death, he presided at the meeting of the British Medical Association, held in Edinburgh in August 1858. The enthusiastic reception accorded to him on this occasion by many hundreds of practitioners from all parts of the kingdom, showed how deep a hold he had gained on the sympathies of the members of that profession, to the service of which, next to God and to the poor, it may be said his active life was entirely devoted.

This was his last appearance in public. It is not the design of this Memoir to enter into the details of private life or the records of the progress of disease. It is, we believe, probable that the materials which exist in the hands of his most intimate friends will be made available to present to the public a more complete picture than can here be attempted, of a life which, in all its varied aspects, is eminently worthy of study.

Our purpose has been chiefly to trace the events of his public career, without more of commentary than was necessary to make the narrative intelligible to every reader. We shall have failed, however, even in our limited object, if we have not conveyed some impression of the high moral dignity which was in Dr. Alison associated with an intellectual power that placed him at the head of his profession. It was the sense universally and justly entertained of his entire self-devotion to the great objects of his life, that lent to his advocacy of them so much weight with all ranks and conditions of men. Otherwise, there was much that would have tended to neutralize his influence. He had none of the habit of self-assertion, which in some men passes for greatness. He was deficient in humour, and hardly aimed at any ornaments of style or manner. His personal conduct was singularly quiet and unobtrusive; and his accessibility to all classes exposed him to the proverbial danger of being underrated through familiarity. Yet no one ever met Dr. Alison, at the bedside, in the lecture-room, in the heat of controversy, or amid the relations of common life, without being impressed with the sense of something more heroic and exalted than was to be found in ordinary men. And the secret of it was, that there *was* in him an element of character that raised him far above the level of the crowd, without one whit impairing his sympathies, or choking the genial hearty humanity that was in him. He had accustomed himself to think that the great ends of life were not to be sacrificed to small and personal considerations. He was not wanting in ambition, but he never hesitated to sacrifice ambition to the desire of doing real good.

He might have been rich; but he preferred a useful and generous expenditure to the accumulation of wealth. He might reasonably have given up his time, even on the score of duty, to those who placed a pecuniary value upon it. But no consideration outweighed with him the sense of duty and of responsibility with which he gave himself to the cause of the unprotected and neglected poor. To them he was, to the last, a being from another world, as it were, come to do them good. And truly, in the pursuit of this, his great duty, he seemed to move in an empyrean above all sordid considerations of self-interest, and far removed from every form of low and mean passion.

We have barely touched on one aspect of Dr. Alison's character, which was, nevertheless, the best and highest aspect. A calm and deep religious faith ruled all his thoughts, and was the master-key to all his motives of action. He was not, indeed, one of those who talked much about religion; he only acted it. Nevertheless, when much moved by human sufferings, or by indignation at human wrongs, his mind naturally reverted to that court of highest appeal, by which all his own impulses were directed to good; and he did not hesitate to plead for his fellow-mortals in the name of that God whom they all served, and that Master who had charged His disciples to remember that they were all brethren. In his latter years, when the world was gradually retreating from his view, the flame of a heart-felt piety seemed to shine through everything he did or wrote; and some of the very last of his contributions to medical literature were addressed to questions regarding those higher mysteries of being which will only be completely revealed to any of us when we have followed him "within the veil."

INDEX.

ADDRESS, Presidential, British Medical Association (Glasgow), 1; in Medicine (Dublin), 66.

Alcohol as food and as stimulant, 211, 214.

Alcoholic stimulation, in epidemic fevers, 110, 115; declaration against, 116 *note;* inquiry on the subject, 118; in typhus fever, 122 *et seq.;* in epidemic of 1826-29, 154; limits of, in acute disease, 208.

Aliena mens, the, 264.

Alison, Rev. Archibald, father of Dr. W. P. Alison, 389, 390.

Alison, Dr. W. P., memoir of, 388; early education 389; social circle in Edinburgh, 390; Professor Dugald Stewart, 391; graduation, travels, dispensary work, 393; early impressions, and writings on epidemic fevers, 395, 400; professor of medical jurisprudence, 400; of Institutes of Medicine, 401; writings, 402-405; increase of epidemic fever, 406; attack upon the Scottish system of dealing with the poor, 411; reaction against depleting practices in fevers, 112-114, 125, 126, 145; his opinions in 1827, 155-157; his practice as regards stimulants in typhus fever, 226, 229; his *Observations on the Management of the Poor in Scotland*, 417; controversy following, in which Rev. Dr. Chalmers took part, 418; Royal Commission and Report, *ibid.;* legislation in 1845, *ibid.;* succeeds Dr. Home in Chair of Practice of Physic, 423; observations on epidemic fever of 1843, 424; correspondence with Dr. Farr, *ibid.;* first shock of illness, 425; resigns the Chair of Practice of Physic, 426; controversy with Dr. Bennett, 427; president of British Medical Association (1858), 427; personal character, 428; religious faith, 429.

Allan, Dr., of City of Glasgow Fever Hospital, on milk diet in fevers, 187.

"Allopathy," 318, 325.

Amyl-nitrite, 95.

Anaxion, the case of, as illustrating blood-letting by Hippocrates, 83, 84 *note.*

Anstie, on stimulants and narcotics, 215.

Antimony, its supposed introduction by Basil Valentine, 97, 382; controversies in the Faculty of Medicine of Paris, 378; the lesson of the past, 381.

Antipyretic treatment, 91.

Antisepticism, 90.

Apothecaries, Company of, 17, 56.

Apprenticeship, under the Medical Act of 1815, 56.

Aquinas, St. Thomas, 13.
Arabian physicians, 8, 43 *note.*
Aristotle, 7, 16.
Arnold, Matthew, and the *Zeit-Geist*, 67.
Asclepiades, of Bithynia, his criticism of Hippocrates, 10.
Astrology, 13.
Atheism, not always a real reproach, 41.
Averroes, Arabian physician, 43 *note.*

BACON, Lord, anticipation of by Hippocrates, 4.
Bacon, Roger, 13, 41, 386.
Beef-tea, of inferior value to milk, 238.
Begbie, the late Dr. Warburton, his address in medicine, 70; his article on Belladonna as a prophylactic, 344 *note.*
Bible, the, 42, 50, 52, 54.
Bichat, new birth of pathological anatomy, 80.
Black, Joseph, the discoverer of latent heat, Professor in the University of Glasgow, 21, 24.
Blackie, Lord Provost, and the Glasgow Improvement Act, 288.
Blood-letting, the case of Anaxion, 83, 84 *note;* sectarian opinions about, after Hippocrates, 85; in acute diseases, 102; Dr. James Gregory on, 103-108; in fevers, 113, 155.
Board of supervision in Scotland, in reference to administration of poor-law, 418, 419.
Boerhaave, 86, 87.
British and Foreign Medical Review, 102 *note.*
British Medical Association, Presidential address on the Physician as Naturalist, 1; address in Medicine, on advances during a hundred years, 66.
Bromide of potassium, 94.
Broussais, 88 *note.*
Brown, Dr. John, author of *Rab and his Friends*, 80 *note.*
Brown, Dr. John, and Brunonianism, 78, 80, 87 *note*, 119.

CAELIUS AURELIANUS, 73.
Carpenter, Dr. W. B., 23 *note.*
Carrick, Mr. John, and the Glasgow Improvement Act, 288.
Causes of Disease, study of, 100, 268.
Celsus, 73.
Chalmers, Rev. Dr., his views on the poor-law opposed to those of Dr. Alison, 417.
Chaucer, *Canterbury Tales*, referred to, 1, 42, 43, 50, 54.
Chicken-pox and modified small-pox, Dr. Alison's experience, 399.
Chloral-Hydrate, 94.
Chloroform, 93.
Cholera in Glasgow (1866), 281-83.
Christianity, 52.
Christison, Sir Robert, on epidemic fever, 112, 157; his notes of Dr. James Gregory's lectures on blood-letting, 103-108; his address in therapeutics, 120 *note.*
Cod-oil in phthisis, 92.
Commission, Royal, on Schools (1861), 29, 33 *note.*
Commission, Royal, on Scientific Instruction (1872), 30, 33 *note.*
Confession, the Westminster, 53.
Conolly, Dr., his reforms in the management of the insane, 257, 262.
Creeds of the Church, 52.
Crisis in fevers, 161 *et seq.;* in typhus, 170, 194 *et seq.*, 201-204.
Cullen, William, 21, 24; and his age, 74-76; and Brown, 77; instability of the medical art in his time, 86.
Cure and prevention of disease, closely allied functions, 277; ought not to be separated, 278.

DARWIN, CHARLES, 47-49.

Delarive, his visit to the Retreat at York, 256.

Delirium tremens, treatment of, 262.

Democritus, an illustration of the method of Greek philosophy, 385.

Demoniacal possession, 264.

Digitalis, 95.

Doctour of Physike, Chaucer's, 1, 11, 42, 50.

Dodoens, Rembert, botanist, 386.

"Double Qualification," the, 16.

ERASISTRATUS of Alexandria, 7; an opponent of blood-letting, 85.

Evolution of the medical art, 67.

FARADAY, his evidence, in 1862, on scientific training in schools, 33.

Faculty of Medicine in Paris, 16, 375; condemns antimony in two formal decrees, ratified by the Parliament of Paris (1566 and 1615), 378; formally allows antimony (1666), 380.

Famine in 1846, Dr. Alison on, 424.

Fayrer, Sir Joseph, on milk diet, in chronic tropical diarrhœa, 189.

Fever and destitution, their connection in Dr. Alison's mind leads to his advocating a poor law, 406 *et seq.*

Fever, epidemic of 1843 in Scotland, observations on, by Dr. Alison, 424.

Fevers, epidemic, Dr. Alison's earlier experience in, 396 *et seq.*; eruptive typhus rare in 1817-19, 397; increase of typhus after 1830, 407.

Fevers, epidemic, treatment of, 111, 154, 199.

Forbes, John, M.D., Editor of *British and Foreign Medical Review*, 102 *note*, 258.

Fuchs, Leonard, botanist, 386.

GALEN, on Hippocrates, 7, 8, 9; his eclectic system, 73, 74, 85.

Galileo, 41, 386.

Gesner, Conrad, naturalist, 386.

Glasgow, a decennium of sanitary work in, 280 *et seq.*; cholera in (1866), 281; small-pox in (1871), 283; typhus in, 284; rookeries in, 286; Improvement Act, 288.

Graves, Dr., of Dublin, quoted, 20; his treatment of fevers, 113, 228.

Gregory, Dr. James, Professor in Edinburgh, his lectures in 1819, 103; his opinions on blood-letting in pneumonia, 105, 108; uncle of Dr. Alison, 390.

Guénaut, champion of antimony in the time of Molière, 378, 381.

HAHNEMANN, 315; his statement of the homœopathic position, 317; characteristics, 321; his letter to Hufeland, 327; his philosophy of disease, 328; and of cure, 331; critical remarks on, 333 *et seq.*

Hamilton, Sir Wm., professor in Edinburgh University, his question as regards the advance of the art of medicine, 70.

Harvey, and Lord Bacon, 6; and the College of Physicians, 16, 19; circulation of the blood, 371, 377.

Henderson, Dr., his statement of Hahnemann's position, 327; on the psoric hypothesis, 354; on the Vienna statistics, 358; on the "necessary limitations" of homœopathy, 364.

Hering, his admission that diseases are aggravated by homœopathic remedies, 353.

Hippocrates, 4; on the physician as the servant of nature, 7-9, 260 *note;* progress in the art since, 70; difficulties in estimating this, 72, 73; his practice in respect of blood-letting, 83, 84 *note.*

Hoffmann, Frederick, Professor in Halle, 86.

Holmes, Oliver Wendell, on medical tradition, 51.

Home, Dr., professor of the Practice of Medicine, death of, 423.

Homœopathy, 315; its exclusiveness, 317; does not admit of compromise, 319; what has to be proved, according to these assumptions, 320; Hahnemann's polemics, 321; examination of the H. law, 326; exceptions, 329; objections to the argument, 333; "like" and "unlike," fallacies underlying, 338; illustrations, 340; quinine and ague, 342; belladonna and scarlet fever, 343; vaccination and small-pox, 344; logical fallacies, 346; examination of the H. practice, 348; infinitesimal doses, 350; Hahnemann a witness against his own system, 353; the psoric hypothesis, 354; homœopathic statistics, 358; Hahnemann's place in history, 363; present-day homœopathy, 364; its opposition to scientific medicine, 366, 368.

Hooker, Sir W. J., 33 *note*.

Hope, Dr. Thomas C., discoverer of the law of expansion of water, professor in the Universities of Glasgow and Edinburgh, 22.

Hufeland, Hahnemann's letter to, 327, 331.

Hunter, John, 65.

Hygienic remedies, 98.

Hyper- and hypo-sthenic diseases and remedies, 81.

JENNER, SIR WM., on milk diet in fever, 186.

KENNEDY, DR. HENRY (Dublin), 212, 235.

Lacte non vino, 110 *note*, 119.

Lancet, on alcoholic stimulation, 149.

Le Clerc, *Histoire de la Médecine*, 10 *note*.

Linacre, Thomas, founder of the Royal College of Physicians of London, 18, 19.

Louis XVI., treated with antimony by Guénaut, 379.

Lyell, Sir Charles, 33 *note*.

MACEWEN, DR. WM., his researches in cerebral pathology and surgery, 314 *note*.

M'Kendrick, Professor, his address on physiology, 26.

Magic, natural, in Chaucer, 12.

Mazarin, Cardinal, submits to antimonial treatment, 379.

Medical Act of 1815, 56; of 1858, 1859, 60, 61.

Methodists, their medical doctrines, 73.

Milan, incident in the great hospital there, 81.

Milk in fevers, as a diet, 118, 167, 184, 188; in chronic diarrhœa, 189.

Mind and body, address to the Medico-Psychological Association, 245.

Modern treatment of the insane, 252.

Molière, 13, 14, 15, 308, 362, 375, 376, 381.

Montaigne, 13, 375.

Morgagni, John Baptist, 87; his great work, 302; its influence on the theories of disease, 303; its spirit and method, 305; his predecessors and successors, 307; his relation to modern progress, 311; and his successors, address on Pathological Science, 294.

Mortality in typhus, statistics of, 135, 146.

Murchison, Dr., on statistics of mortality in typhus fever, 134; on diet in fevers, 184; on crisis, 194.

NARCOTICS, 94.

Natural History as a discipline, 384.

"Naturalist," the Physician as, 1.

Naudé, Gabriel, *Apologie pour les grands hommes*, etc., 12; on judicial astrology, 13.

Nitroglycerine, 95.

Novum Organum, the, anticipation of by Hippocrates, 4.

Organon of the healing art, Hahnemann's, quoted, 315 *et seq.*

Orthodoxy, old, and modern opinion, present aspect of medical science, 51, 98, 370 *et seq.* Orthodoxy and heresy in the olden time, 375.

Over-stimulation, Dr. Todd's views on, 221; author's views on, 240.

Owen, Richard, naturalist, 33 *note.*

PARACELSUS and Hahnemann, 323.

Paraldehyde, 94.

Pathological and Clinical Society of Glasgow, its origin, 295; objects, 297-302.

Pathological Science, progress of, address to the Pathological and Clinical Society, 294.

Pathological Society of London, 299.

Pathology and Practice of Medicine, Outlines of, by Dr. Alison, 423.

Patin, Guy, dean of the Faculty of Medicine in Paris, 13, 14, 375 *et seq.*

"Physic," an example of the degeneration of words, 2, 3.

Physics, study of, imperfectly recognized in medical education, 27, 28; in general education, 29.

Physician, the, as "Naturalist,"1; as "naturæ minister," 5; in the Middle Ages, 11.

Physicians, Royal College of, 17, 18, 19.

Physiology and Pathology, Outlines of, by Dr. Alison, 402, 403.

Pinel, his reforms at the Bicêtre, 255, 262.

Playfair, Sir Lyon, on science in schools, 31, 32; on sanitary science, 270.

Polypharmacy, 21.

Poor-law Act, Scotland (1845), 418.

Poor in Scotland, Dr. Alison on the management of the, 417; Royal Commission and legislation following, 418.

Poor-laws in Scotland, Report by General Assembly in 1818, 408; contrast with England, 410; Dr. Alison attacks the Scottish system, 412.

Preventive medicine, 98.

Pulse, in typhus, 164, 171-174.

RABELAIS, 13, 375.

Rasori, Italian physician, 81.

Realism and nominalism in the Middle Ages, 385.

Registration of births, deaths, and marriages in Scotland, Dr. Alison's correspondence with Dr. Farr, 424.

Religio Medici, 40-54.

Richardson, Dr. B. W., on John Brown, 78 *note.*

Royal Commission on the poor-laws, in Scotland, reported in 1844, 418.

SALICYL compounds, 95.

Sanitary science and preventive medicine, address to the Sanitary Institute of Great Britain, 266.

Sanitary work in Glasgow, a decennium of, 280 *et seq.*

Science, modern, its alleged tendencies, 25; deficiency of, in schools, 33-39; and religion, 49.

Scurvy, 269, 270.

Sects after Hippocrates, their influence, 73.

Self-healing process in disease, 101.

Simpson, Sir James Y., quoted in reference to infinitesimal doses, 351.

Small-pox in Glasgow (1871), 283.

Specialism in medicine, 249.

Stahl, Ernest, Professor in Berlin, 86.

Statistics of mortality in typhus, 135.

Stewart, Dr. A. P., on crisis in fever, 194.

Stewart, Professor Dugald, 390, 391.

Strophanthus, 95.

Surgeons, Royal College of, 17.

Sydenham, neglect of sanitary research by, 269.

THERMOMETER as a guide to prognosis, etc., note on, 191.

Times, The, referred to. 55, 56, 59.

Todd, Dr., 124-126, 212, 214 *et seq.*

Tommasini, Professor in Bologna, 81.

Treatment of fever, 199.
Tuke, Dr. Daniel Hack, 246 *note;* 257 *note.*
Tuke, Wm., and the Retreat at York, 255.
Type, change of, in epidemic fevers, 112, 154.
Typhus fever, alcoholic stimulation in, 127 *et seq;* statistics of mortality in, 135, 146, 232; epidemic of 1826-29, 154; course of, and phenomena of the crisis, 158 *et seq.*, 194 *et seq.;* treatment, 179, 199, 204-207.

URE, Lord Provost, an ardent sanitary reformer in Glasgow, 279 *note.*
Urethan, 94.
Universities, the English, 17, 19.
Universities, the Scottish, 20-24, 61.

VALENTINE, BASIL, 97, 382.
Valsalva, his relation to Morgagni's great work, 305.
Vesalius, of Brussels, anatomist, 386.
Vienna Homœopathic Hospital statistics, 358-361.
Virchow, his *Cellular Pathology*, 313.
Vital affinity, writings on, by Dr. Alison, 403.

WELSH, Dr., on blood-letting in fever, 113 *note*, 155, 229.

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